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STATED AND REVEALED PREFERENCES AFTER A DISASTER: IDENTIFYING PERSISTENT TIME PREFERENCE SHOCKS

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Identifying such shocks is challenging: stated preferences are sensitive to economic conditions, and revealed preference approaches often require structural assumptions that are unlikely to hold after extreme events. We propose a theory-based strategy that jointly analyzes stated and revealed preferences to distinguish preference shifts from constraint-driven behavior. An impatience shock should increase both stated impatience and consumption; a constraint shock should increase stated impatience but reduce consumption or savings. We apply this strategy to the 2009 L'Aquila earthquake in Italy, combining repeated preference elicitation, household panel data, and granular seismic exposure. We exploit quasi-random variation in shaking intensity among undamaged households. Exposed individuals report greater impatience and increase consumption despite income declines, leading to persistent draw-downs in liquid assets. These findings suggest that psychological exposure to trauma can generate durable preference shifts that reduce long-run financial resilience. As climate-induced disasters become more frequent, this behavioral margin may play an increasingly important role in shaping economic vulnerability.

KEYWORDS: Time preference, consumption, natural disaster, Italy.

1. INTRODUCTION

You ask people questions, but you want to be sure that you interpret correctly what they are saying.
(Orazio Attanasio[1])

Identifying preference shocks is challenging because they are not directly observable. Researchers often resolve this challenge by inferring preferences from actual choices - a revealed-preference approach that is valid under the assumption of stable preferences.(e.g., Attanasio and Weber (1995b), Gourinchas and Parker (2002)). However, a growing body of evidence suggests that preferences may change following major shocks such as natural disasters or episodes of violence (e.g., Voors et al. (2012), Cavallo et al. (2013), Callen et al. (2014)). These studies rely on stated preferences, elicited through hypothetical choices, which are easier to collect in the aftermath of a shock and better suited to capturing transient changes in preferences.

Yet stated preferences come with their own limitations: responses are systematically biased by economic conditions.[2] Households facing income loss or liquidity constraints often appear

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[1] 16 March 2023 in the "Coffee with..." interview of the FAIR Insight Team

[2] See Almås et al. (2024) for a discussion of these biases and Appendix A1 for a survey of related papers.

more impatient, not because of changes in underlying preferences, but because such constraints distort their responses to hypothetical scenarios (e.g., Guiso et al. (2008), Haushofer and Fehr (2014), Carvalho et al. (2016), Epper et al. (2020)). This creates a core identification problem. Natural disasters offer a compelling empirical setting: they are large, salient, and plausibly exogenous. But they also induce a shock to economic constraints through income losses, asset depletion, or tighter credit, which may bias stated-preference measures. Disentangling genuine preference shocks from constraint-driven responses remains a central challenge in this literature.

We propose an identification framework that distinguishes preference shocks from constraint-driven responses by jointly analyzing stated and revealed time preferences. The logic follows directly from a standard consumption–savings model. A genuine increase in impatience should raise both stated impatience and current consumption. In contrast, tightened economic constraints also raise stated impatience, due to bias in hypothetical responses, but reduce consumption or savings (see Carroll and Kimball (1996)). This divergence in predictions enables us to infer time preference shocks without imposing strong structural assumptions. This logic parallels classic identification strategies in macroeconomics, where demand and supply shocks are distinguished by observing whether prices and quantities move in the same or opposite directions. Here, stated and revealed preferences serve as analogous observables to price and quantity. We apply this framework to households affected by the 2009 L'Aquila earthquake, and document a persistent increase in true underlying impatience.

We are not the first ones to combine hypothetical questions and actual choices, a theme emphasized in the 2024 Econometric Society Presidential Address (Almås et al. (2024)). Recent contributions, such as Bernheim et al. (2021) and Meango et al. (2025), develop new estimators that assume a stable mapping between hypothetical and actual choices.[3] Our setting differs in that we do not impose stability in this mapping. Instead, our identification strategy exploits the possibility that stated and revealed preferences may diverge, depending on whether changes are driven by underlying preference shocks or by economic constraints, and uses this divergence to identify latent preference shocks under minimal assumptions.

To implement our identification strategy, we rely on data that is rare to come by: the Bank of Italy's Household Income and Wealth Survey (SHIW) elicited both stated preferences and behavior before and after the L'Aquila earthquake. The SHIW includes repeated, experimentally validated time preference measures and rich panel data on household consumption, income, and liquid asset holdings. This allows us to follow stated and revealed preferences within the same households over time. We link these with geospatial and seismic records to capture local shaking intensity and structural damage at the municipality (comune) level. Our identification strategy requires fine-grained, people-based panel data,[4] where both exposure and behavior are observed at the household level. Such spatially and temporally granular data are increasingly used in disaster studies for improved identification (e.g., Deryugina et al. (2018), Tellman et al. (2021), Nakamura et al. (2022)).

We study the evolution of stated preferences and observed behavior over time, with difference-in-differences and event study models. These designs allow us to isolate genuine preference shocks from responses to changing economic constraints without imposing functional form assumptions. Our identification strategy exploits two distinct sources of quasi-random variation, generated by the 2009 L'Aquila earthquake. First, the timing of earthquakes is unpredictable, and in this case, the epicenter location was unexpected as well, in a region

[3] For complementary work combining hypothetical evaluations with actual choices, see, for example, Giustinelli and Shapiro (2019), Wiswall and Zafar (2015), Andrew and Adams (2025)

[4] Terminology borrowed from Deryugina et al. (2018).

not previously classified as high-risk. Second, seismic waves propagated quasi-randomly, with some municipalities experiencing severe shaking while others at a similar distance from the epicenter experienced none. This creates a form of double randomization by epicenter location and local intensity, enabling clean comparisons across households with differential exposure but otherwise similar characteristics.

Many studies rely on the first layer of randomization, using distance from the epicenter as a proxy for disaster exposure. Instead, we leverage both the distance from the epicenter and the seismic movement and damage data to construct treatment and control groups. We define treated households as those located in municipalities with intense ground motion but whose homes remained structurally intact, what we term the *shaken but not devastated* group. We *exclude households with damaged homes* because severe destruction likely disrupts standard consumption-savings behavior, making theoretical frameworks based on intertemporal optimization less applicable. We employ two control groups: (i) households in unaffected regions, to account for national trends and examine spillover effects close to the epicenter; and (ii) a *spatial control* group, including households that experienced no ground motion, but live at similar distances from the epicenter as the shaken but not devastated group. This latter group helps account for local economic spillovers, such as job losses, price shocks, or indirect exposure through social networks. As a test of this, we show that disposable income in the treated and spatial control group evolved similarly, suggesting that differential economic constraints cannot account for the divergence in their stated and revealed preferences.To examine changes in behavior, we define forward-looking households - those able to smooth consumption intertemporally - as those with sufficient liquidity.[5]

We document substantial and persistent increases in latent time preference: after the earthquake, individuals in shaken but not devastated municipalities report significantly lower stated discount factors and increase nondurable consumption by €90–160 per month, a 10% rise. These effects are not short-lived. We observe stated time preferences for three years post-earthquake, during which consumption stays elevated. We follow households' consumption-saving choices for an additional four years, during which we find sustained reductions in savings and increased reliance on debt. As a result, shaken households dropped by nine percentile points in the national distribution of net liquid wealth. These results are robust to controls for time-varying income, liquidity constraints, interview timing, switches in within-household respondents, and adjustments for the curvature of the utility function.

At a broad level, our findings are consistent with prior work documenting shifts in stated time preferences following traumatic events (e.g., Voors et al. (2012); Callen et al. (2014); Chantarat et al. (2016); Cassar et al. (2017)). Much of this literature acknowledges the difficulty of separating genuine preference shifts from changes driven by economic hardship, therefore interpret stated preference changes as contextual behavioral responses. We move beyond the approach of using stated preferences (and conditioning on economic circumstances), and develop an identification strategy based on the joint behavior of stated and revealed preferences, that allows us to infer latent preference shocks. Second, we complement this literature by leveraging high-resolution seismic and damage data to measure psychological exposure without damages, and to net out indirect, and general equilibrium spillovers.

We explore several candidate mechanisms for the observed preference shifts. While short-term risk preferences briefly rise post-disaster, which could impact our estimates (see Andreoni and Sprenger (2012); Andreoni et al. (2015)), they return to baseline within two waves and do not explain the long-run behavioral changes. Nor can belief updating about survival probability

[5]Non-hand-to-mouth (non-HTM) households are defined as having liquid savings higher than one week of income or wealth more than six months of income (see Zeldes (1989),Kaplan et al. (2014), and Aguiar et al. (2025)).

(Falk et al. (2019)) account for the magnitude of the effect: reconciling the increased impatience would require halving expected lifespan, an implausible adjustment. Nor beliefs about disaster risk, because we do not see an increase in disaster insurance (Gallagher and Hartley (2017)). The evidence instead points toward a psychological stress mechanism, consistent with laboratory studies linking stress to the salience of near-term outcomes and narrowing cognitive scope for future-oriented thinking (Lerner et al. (2013),Cornelisse et al. (2013)).[6] We test this by examining heterogeneity: groups more vulnerable to stress exhibit the strongest behavioral shifts.

That these effects arise in a high-income setting, where disaster insurance, credit access, and public safety nets are well-developed, underscores the capacity of psychological trauma to generate persistent shifts in intertemporal preferences, even absent material loss.[7] If such shocks occur more frequently, and more broadly, as is likely under climate change, they may reshape saving behavior, widen inequality, and complicate welfare analysis.

The paper is organized as follows. The next section describes our conceptual framework for identifying time preference shocks. Section 3 covers the empirical setting and data. Section 4 covers the empirical design. Section 5 provides the estimation results. Section6 discusses potential mechanisms. Section 7 concludes.

2. CONCEPTUAL FRAMEWORK

2.1. *The problem: bias in stated time preferences*

We are interested in the effect of a non-staggered treatment on the true underlying time preferences. To fix ideas, suppose we elicit stated time preferences $\tilde{\beta}_{i,t}$ as discount factors from a set of individuals $i \in \mathbb{S}$ using hypothetical "money earlier or later" (MEL) choices, where subjects choose between smaller, immediate monetary rewards and larger, delayed rewards. The true discount factors $\beta_{i,t}$ vary across individuals and over time, but are unobserved.

A primary problem with MEL elicitation is that they are biased measures of the true time preferences. Having low income or liquidity, even temporarily, causes lower stated patience in MEL answers (Carvalho et al. (2016)).[8] At the same time, MEL elicitations do contain valuable information about intertemporal choices. Those with lower stated patience tend to save less (Falk et al. (2018)), accumulate less wealth (Epper et al. (2020)), invest less in human capital (Sunde et al. (2022)), and search shorter for high-paying jobs (DellaVigna and Paserman (2005)). We formalize these empirical relationships with the following bias function, a reduced-form measurement equation:

$$\tilde{\beta}_{i,t} = B(\underset{+}{Y_{i,t}}, \underset{+}{X_{i,t}}; \beta_{i,t}), \tag{1}$$

[6] For a summary see Haushofer and Fehr (2014). For a psychological background see Lazarus and Folkman (1984). Related theoretical work on salience (Gennaioli and Shleifer (2010), Bordalo et al. (2012)) show how attention to salient features of the environment can distort decision weights. Though originally developed for risk, similar limited attention principles have been extended to intertemporal choice and myopic discounting (Gabaix and Laibson (2021)).

[7] Most earlier studies focus on developing countries, with a handful of papers in developed country context, focusing on damaged households, like Hanaoka et al. (2018), Akesaka (2019).

[8] Guiso et al. (2008) was among the first to highlight how economic constraints shape responses to hypothetical questions in the context of risk perception and portfolio choice. More recently, researchers have proposed ways to estimate the bias and combine stated and revealed preferences for causal inference or structural estimation (Bernheim et al. (2021); Meango et al. (2025)).

where the stated discount factor $\tilde{\beta}$ increases in income $Y_{i,t}$, and in the true underlying discount factor β, with plus signs indicating these correlations. $X_{i,t}$ collects other correlates, such as assets, education, age, and family size. β is unobserved, and it is the object of interest.

2.2. *Theory-Based Identification of Latent Preferences*

The bias in stated preferences complicates identification: changes in individual stated time preferences may reflect true time preference shifts, changing economic conditions, or both. Our approach is to use economic theory to map the bias and true preference changes to actual behavior. As a second step, we use the spatial propagation of our treatment to tackle remaining concerns, such as indirect or general equilibrium effects.

2.2.1. *General identification logic*

We identify time preference shocks by exploiting a theoretical prediction: genuine changes in impatience and changes in economic constraints produce opposite movements in consumption. In the canonical intertemporal consumption-savings model, agents maximize lifetime utility, allocating resources across periods using a risk-free asset with return $(1+r)$, with a temporal budget constraint

$$\max \sum_{t=0}^{\infty} \beta_{i,t}^{t} u(C_{i,t})$$
$$A_{i,t+1} = (1+r)A_{i,t} + Y_t - C_{i,t},$$

where $A_{i,t}, Y_t, C_{i,t}$ are assets, income, and consumption of agent i at time t, while $\beta_{i,t}$ is the agent's potentially time-varying discount factor. The optimal intertemporal choices are described by the Euler equation:[9]

$$u'(C_{i,t}) = \beta_{i,t} E_t (1+r) u'(C_{i,t+1}). \tag{2}$$

For a simple case of log-utility, the policy function has a closed-form solution. Initial consumption is a function of discounted lifetime income Y

$$C_{i,0} = \frac{1}{1+\beta} \sum_{t=0}^{\infty} \frac{Y_{i,t}}{(1+r)^t} = \frac{1}{1+\beta} Y. \tag{3}$$

Figure 1 uses this simple example to illustrate the two cases for which stated time preferences change the same way: an income decline and a decline in the true discount factor. In the first case, income declines from Y to Y', the respondent states a lower stated discount factor due to the bias in stated preferences, and consumption shifts downward along the existing policy function.[10] In contrast, a decline in β reduces the stated discount factor, but pivots the policy function upward, increasing immediate consumption, assuming an unconstrained choice. Identification of time preference shocks relies on comparing two similar individuals who earn similar income and make similar consumption choices before the event. If the treated individual

[9] In the Euler equation, we implicitly assume that $\beta_{i,t} = E_t \beta_{i,t+1}$.

[10] Conditional on income, we might observe unchanged consumption, if income entirely captures the bias. However, if the budget constraint becomes binding, we would still see a decline in stated discount factor (see Carvalho et al. (2016)).

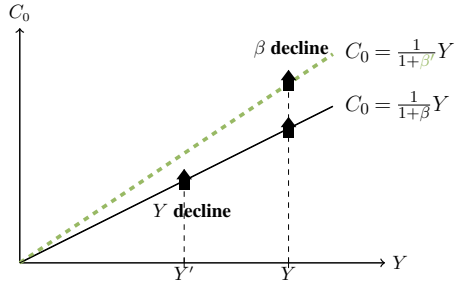


FIGURE 1.—Consumption in the first period C_0 as a function of discounted lifetime income, assuming log-utility.

states a lower stated discount factor after the event and increases consumption, this identifies a decline in the true underlying discount factor.

We can identify time-preference shocks in a similar manner for general utility functions. Although in this case there is no closed-form solution, the properties of the consumption policy function are well established in both theory and empirical work (see, e.g., Attanasio and Weber (1995b), Carroll (1992), Carroll et al. (2021),Aguiar et al. (2025), Commault (2022)). We summarize these in equation 4: a decline in income weekly reduces consumption (indicated by $0+$), depending on the persistence of the income shock and the agent's ability to smooth consumption. By contrast, lower patience β weakly increases current consumption, as future utility is less valued.

$$C_{i,t} = C(\underset{0+}{Y_{i,t}}, X_{i,t}; \underset{0-}{\beta_{i,t}}). \tag{4}$$

The identification strategy requires households to be on their Euler equation. Those whose budget constraint is binding, i.e., are hand-to-mouth (HTM), would not be able to increase consumption in response to a decline in $\beta_{i,t}$. In other words, their revealed preference about intertemporal choices cannot be identified because they do not make intertemporal choices, but simply consume their income. A time preference shock for a non-constrained household (Kaplan et al. (2014)) is identified as an impatience preference shock if their stated discount factor declines while their consumption increases at the same time.

$$sign(\Delta\tilde{\beta}_{i,t}) = -sign(\Delta C_{i,t}). \tag{5}$$

2.3. *Application to natural disasters*

Natural disasters often cause significant direct damage, which complicates identification. Naturally, those whose houses were damaged would be more willing to pay a higher price for immediate payment in a MEL question, because they need liquidity, not because they became more impatient. In addition, the circumstances of damaged households have changed so much that they are likely not on their Euler equation. Therefore, in our estimations, we exclude households with damaged houses (red and yellow on Figure 2), using *geocoded seismic and damage data*, allowing us to focus only on those who were indirectly affected.

We partition households based on location r_i: $\mathbb{S} = \mathbb{S}_T \cup \mathbb{S}_C$. Treated households ($r_i \in \mathbb{S}_T$) are those exposed to seismic shaking without home damage. Control households ($r_i \in \mathbb{S}_C$) experienced no ground motion.

2.3.1. *Spatial refinement for indirect effects*

A second concern is that even undamaged households may face indirect economic losses. Natural disasters can destroy livelihoods, disrupt labor markets, and raise the cost of living, potentially tightening liquidity constraints. Spillover effects may extend far beyond the directly affected area through supply-chain disruptions and regional economic shocks (Barrot and Sauvagnat (2016), Carvalho et al. (2020)). These indirect effects are difficult to control for because they operate through multiple unobservable channels. Moreover, controlling for income creates a "bad control" problem (Angrist and Pischke (2014)): the earthquake affects income, which mediates its effect on stated preferences, so conditioning on income may block part of the causal pathway or introduce bias.

These challenges are specific to the natural disaster itself; thus, we solve them using the *spatial propagation* of the natural disaster. The key insight is that households equidistant from the epicenter likely experience similar indirect and general-equilibrium effects, even if they differ in ground-motion exposure. This motivates a spatial control group: households at similar distances from the epicenter as treated households, but in areas that experienced no shaking.

Let $d(r_i, s)$ denote the distance from household i's location to the epicenter. We augment the measurement equations to include a latent variable $\theta_{i,t}$), capturing indirect effects (infrastructure disruptions, labor market shocks, social obligations):

$$\tilde{\beta}_{i,t} = B(Y_{i,t}, X_{i,t}; \beta_{i,t}, \theta_{i,t}) \tag{6}$$

$$C_{i,t} = C(Y_{i,t}, X_{i,t}; \beta_{i,t}, \theta_{i,t}). \tag{7}$$

The spatial control group is defined as:

$$\mathbb{S}_{C_d} = \{r_i \in \mathbb{S}_C : d(r_i, s) \leq d^*\}, \quad \text{where } d^* = \max_{r_j \in \mathbb{S}_T} d(r_j, s). \tag{8}$$

This selects unshaken households within a radius encompassing all treated households. Spatial proximity implies $Y_{i,t}$ is similar across groups - a testable implication we verify empirically. Similrly unobserved $\theta_{i,t}$ should be similar across groups. Then :

$$\mathbb{E}[\Delta\tilde{\beta}_{i,t}|r_i \in \mathbb{S}_T, Y_{i,t}, \theta_{i,t}] - \mathbb{E}[\Delta\tilde{\beta}_{i,t}|r_i \in \mathbb{S}_{C_d}, Y_{i,t}, \theta_{i,t}] = \mathbb{E}[\Delta\tilde{\beta}_{i,t}|r_i \in \mathbb{S}_T] - \mathbb{E}[\Delta\tilde{\beta}_{i,t}|r_i \in \mathbb{S}_{C_d}]. \tag{9}$$

This approach nets out indirect effects without conditioning on income, avoiding the bad control problem while isolating the direct behavioral impact of seismic exposure.

2.4. *Advantages and Limitations*

Our identification framework offers both methodological advantages and practical limitations relative to existing approaches.

Minimal structural assumptions A key advantage is that identification relies only on sign restrictions derived from standard consumption theory, without requiring parametric assumptions about utility functions, the form of the consumption policy function $C(\cdot)$ in equation (7), or the bias function $B(\cdot)$ in equation (6). Recent work has made progress combining stated and revealed preferences for causal inference (Bernheim et al. (2021)), but typically assumes stable mappings between the two measures, an assumption that requires either structural restrictions or that the economic environment remains unchanged. Our approach exploits the fact that

preference shocks and constraint shocks generate different patterns across stated and revealed preferences, allowing identification through directional predictions alone. This contrasts with structural approaches that must specify and estimate preference parameters (e.g., Gourinchas and Parker (2002)), which may be misspecified following large shocks that fundamentally alter decision environments. Our framework remains valid whether utility is CRRA, CARA, or takes another form, and whether the policy function exhibits precautionary motives or other features. The only requirement is that consumption responds positively to income and negatively to patience among unconstrained households, properties that hold across a wide class of models.

Data requirements This generality comes at a cost: the framework requires joint observation of stated and revealed preferences in panel data, a combination rarely available outside specialized surveys like the SHIW. Most datasets contain either stated preference measures or consumption data, but not both, with repeated observations on the same households.

Sample restrictions Identification relies on households being on their Euler equation; we therefore focus on non-hand-to-mouth (HTM) households. Following Kaplan et al. (2014), we classify a household as HTM if it satisfies either of two criteria: liquid wealth below one week of income or at the borrowing constraint, or total wealth below half of annual earnings. In our analysis on consumption and savings, we exclude households classified as HTM in any pre-earthquake wave, which removes 28 percent of our sample. This restriction also addresses a measurement concern: Carvalho et al. (2016) show experimentally that binding constraints inflate stated impatience through monetary choice tasks, independent of true discount rates. Restricting to unconstrained households ensures that both the consumption increase and the decline in stated patience we observe reflect genuine preference shifts rather than constraint-induced measurement bias.

Qualitative vs. quantitative identification While sign restrictions identify the direction of preference changes nonparametrically, they provide limited information about magnitudes without additional structure.

Shock salience The framework is best suited to large, salient shocks that generate measurable behavioral responses. Detecting preference shifts from smaller or more diffuse events may require alternative methods or longer panels to accumulate sufficient variation.

These features define the scope of applicability: our approach is particularly valuable when researchers observe major shocks in settings with rich household panel data and seek to identify preference changes without imposing strong functional form assumptions, but may be less suitable for analyzing gradual preference formation, severely constrained populations, or settings where only cross-sectional data are available.

3. EMPIRICAL SETTING AND DATA

3.1. *The L'Aquila Earthquake*

In the early morning of April 6, 2009, at 3:32 a.m., a medium-power earthquake ($M_w = 6.3$) struck the city of L'Aquila and nearby municipalities in central Italy. The main shock occurred just four kilometers southwest of L'Aquila at a shallow depth (8-9 km), causing severe

structural damage. Occurring at night, it caught many residents in bed; 309 lives were lost.[11] In the aftermath, the government evacuated affected populations and resettled them in camps and hotels. By winter, the government constructed new anti-seismic housing next to L'Aquila to settle displaced residents.[12] Government aid, including reconstruction funds and subsidies, was distributed to municipalities with substantial damages.[13] Infrastructure repairs were swift, but L'Aquila, once a vibrant gathering place, remained closed for years, undermining social life and economic activity. The government financed the restoration of L'Aquila, but reconstruction was slow (remains incomplete as of 2025) and riddled with allegations of corruption and mafia infiltration. The government paid the construction companies carrying out the work directly, rather than financing households (see Acconcia et al. (2020)).

Figure 2 maps the affected areas and their respective damages. According to the Modified Mercalli Intensity (MMI) scale, which ranges from 0 to 10, locations experiencing widespread structural failure, including collapsed buildings and fatalities, received MMI scores between 8 and 9.5 (shaded red). Locations with moderate damage (e.g., broken windows, chimney collapse, failure of non-reinforced structures) were assigned MMI values between 6 and 8 (shaded yellow). Perceptible shaking without structural damage corresponded to values between 4 and 5 (shaded green). Unshaded municipalities reported no seismic activity. The map illustrates the irregular propagation of seismic waves. Municipalities equidistant from the epicenter experienced markedly different shaking intensities. For instance, Penna Sant'Andrea and Bisenti lie roughly 100 km from the epicenter, yet the former suffered damage (MMI 6), while the latter did not (MMI 5). A nearby municipality, Castiglione Messer Raimondo, recorded no measurable shaking.

This uneven propagation forms the basis of our identification. The earthquake provides two layers of quasi-experimental variation: (1) the exogenous location and timing of the epicenter, and (2) the heterogeneous propagation of seismic energy. The unpredictability of epicenter location is well-supported by several features of the 2009 L'Aquila earthquake. First, while long-run spatial patterns of seismic risk in Italy are understood, the timing and specific location of fault ruptures remain inherently unpredictable.[14] Second, L'Aquila was not categorized as high-risk prior to the event, ranking only as "moderate" (level 2 on a four-point national hazard scale). Third, the region had not experienced an earthquake of similar magnitude in nearly a century, reducing both institutional and individual expectations of a major shock. Finally, the 2009 quake activated previously unmapped fault lines, which were absent from the existing hazard maps and only identified after the event.[15] The second layer of quasi-random variation stems from variation in ground motion intensity among municipalities equidistant from the epi-

[11] Survivors cited a false sense of security from a government press conference days earlier, where officials downplayed the likelihood of a major shock. The Department of Civil Protection convened a seismologist panel largely for public reassurance; statements made contradicted the committee's actual conclusions.

[12] Families could choose between two options: moving to these new houses or accepting a lump-sum payment; most opted for the newly-built houses. The construction was financed using European Union funds allocated for temporary accommodations, but many still lived in these houses during our visit in 2024 (EU Committee on Budgetary Control (2013)).

[13] Municipalities at Modified Mercalli Intensity (MMI) 6 or above qualified for aid under Decreto Legge 28 Aprile 2009, n.39.

[14] Italy's seismicity stems from the slow collision and rotation of the African and Eurasian tectonic plates. Faults slip when geological stress surpasses local thresholds, but predicting where and when this occurs is not currently possible.

[15] These faults were not incorporated into existing hazard maps at the time. After the earthquake, hazard maps were revised, and the earthquake also prompted new investments in seismic monitoring and research, including the National Plan for Seismic Prevention under Law Decree no. 39/2009.

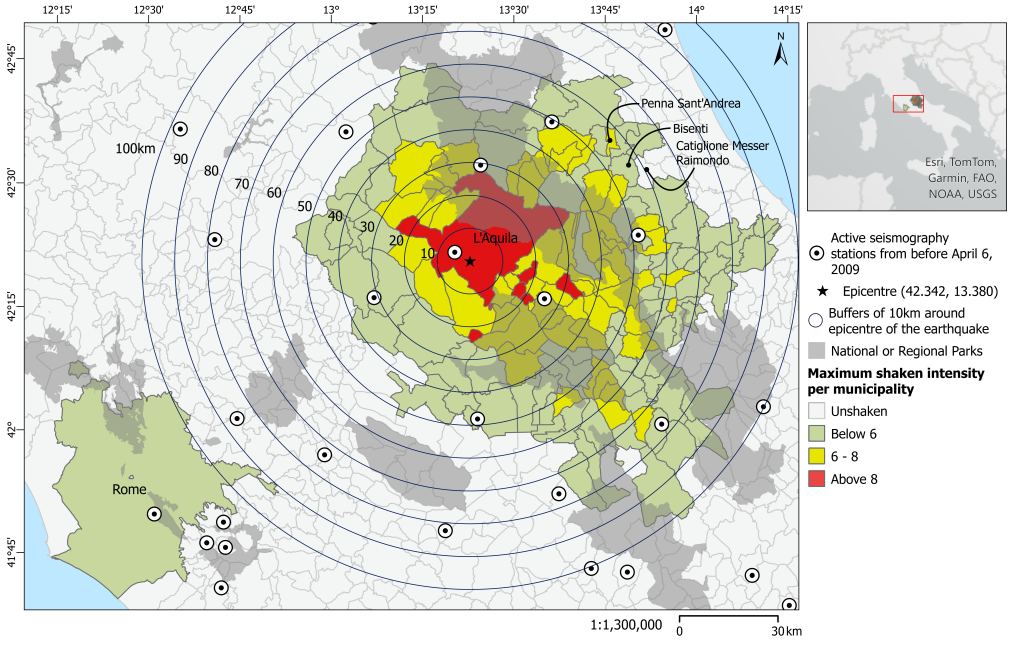


FIGURE 2.—Seismic Activity from L'Aquila Earthquake.

Note: The map was created based on data from the National Institute of Geophysics and Volcanology (INGV), and the Italian National Institute of Statistics (ISTAT). Municipalities with Modified Mercalli intensity scales >8 are severely damaged, 6-8 damaged, 6< are not damaged. Unshaded areas did not have seismic movements.

center. This pattern, understood only post hoc, was driven by localized geological differences, particularly soil composition (Albano et al. (2015), Cantore et al. (2017)).[16]

3.2. *Seismic and Damage Data*

Our earthquake data source is the *Catalogo Parametrico dei Terremoti Italiani*,[17] which gives us information about who felt the earthquake and who was damaged (illustrated on Figure 2). Italy's dense monitoring network gives information about seismic movements and the damage data was collected by the *Quick Earthquake Survey Team* (QuEST).

To ensure the reliability of our data, we contacted QuEST to understand their procedures. Importantly, the location and extent of seismic movement were not inferred ex post: they were observed in real time through seismographs (dots on Figure 2), which provided immediate and spatially precise data on ground motion. This allowed QuEST to prioritize affected areas, cataloguing damage severity, building vulnerability, and structural impacts, which were summarized into MMI scores. We cross-validated and confirmed that no seismically active location is missing from the damage dataset. The combination of objective instrumentation and comprehensive fieldwork makes these data highly reliable.

[16]The shallow depth, amplified surface-level impacts, and differences in soil composition generated differences in the propagation of seismic waves.

[17]Publicly available via the Istituto Nazionale di Geofisica e Vulcanologia (INGV).

3.3. *Household Data*

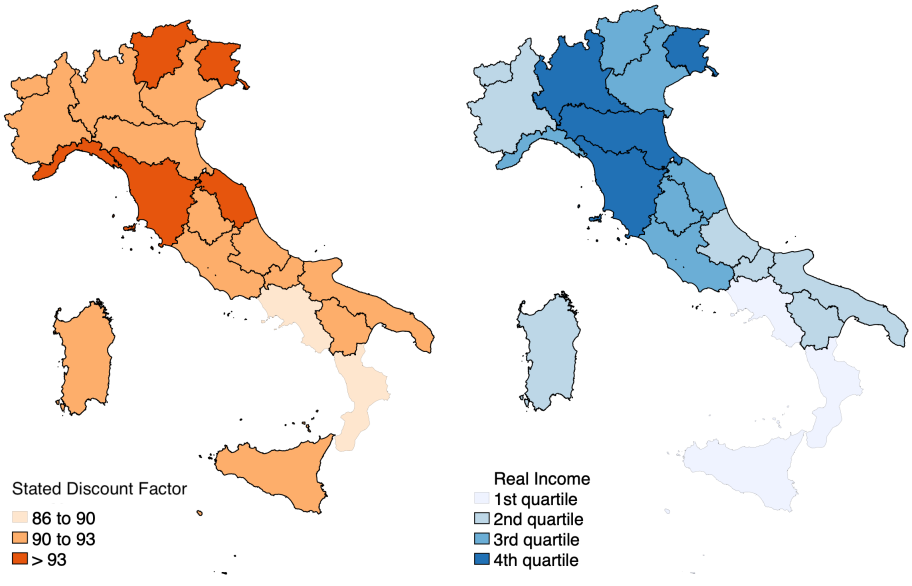
Our primary data source is the Survey on Household Income and Wealth (SHIW), run by the Bank of Italy. Part of a euro area effort on understanding household finance (Survey on Household Finance and Consumption), the SHIW provides valuable data from a representative sample of Italian households. It has been a key resource in life-cycle research due to its high quality, and distinguishing feature, that it elicits income, consumption, and savings within a single survey (see, for example, Guiso et al. (1996), Attanasio and Jappelli (2001), Jappelli and Pistaferri (2010), Jappelli and Pistaferri (2000)).[18] The SHIW surveys 8,000 households biennially, with 50% in a rotating panel.

Our data definitions follow the life-cycle literature. We use disposable income as the primary determinant of consumption (see Blundell et al. (2008)). Consistent with the approach in Attanasio and Weber (1995a), savings is residual income net of consumption (durable and non-durable). Net liquid assets are deposits, bonds, and equity minus outstanding debt. Cash on hand is income plus net liquid assets. We define risky portfolio share following Guiso et al. (1996), and Fagereng et al. (forthcoming). See detailed data definitions in Appendix A.

Time Preferences To identify time preference shocks, we focus on a previously untapped section of SHIW that elicits time preferences via a hypothetical intertemporal trade-off question ("money earlier or later ", see Section A.2). This module was administered in four waves: two waves before the earthquake (2004 and 2008) and two after (2010 and 2012). The panel structure of the data is especially valuable, as it allows us to track stated preferences over time and observe their evolution relative to the earthquake. Although various methods exist for eliciting time preferences, the SHIW question is both simple and experimentally validated, achieving a response rate of 88 percent. One exception is the 2008 wave, when time preference was elicited only for half of the households, for heads born in even-numbered years. For ease of interpretation, we express the responses from the MEL question as implied discount factors, such that a decline in the discount factor reflects a decline in patience (see Appendix C). Time preference data is collected from only one household member, typically the household head. This raises the question whose time preference is most relevant for household intertemporal choices (see Almås et al. (2022)). Rather than taking a position on this, we retain all responses for our baseline estimates, irrespective of which family member was asked, and subsequently assess robustness by restricting the analysis to within-individual variation.

Consistent with the bias documented in the literature (e.g., Haushofer and Fehr (2014); Carvalho et al. (2016); Guiso et al. (2008); Epper et al. (2020)) stated patience in the SHIW correlates strongly with income. Stated time preferences, expressed as discount factors (see Appendix C) are higher in the north where average household income is also higher compared to the poorer South (Figure 3). The same positive correlation holds across individuals (Figure 4): on average, higher income is associated with higher stated patience. This relationship persists when controlling for demographic and financial characteristics (Column 1 of Table I). A 1 percent increase in real income is associated with a 2.4 percentage point increase in the stated discount factor.

[18]The high data quality is ensured by trained enumerators. Its reliability is reinforced by in-person interviews and by allowing households to verify their answers via tax records and bank statements. The survey's representativeness further enhances its quality, achieved through stratified random sampling and the replacement of attrited households with similar ones.



(a) Stated Discount Factor (b) Income

FIGURE 3.—Stated Discount Factor and Income Quartile Across Italian Regions (Pre-Earthquake)

Note: Panel (a) colors the 20 Italian regions by their pre-2009 average stated time preference, and panel (b) by their average position in the real income distribution. Darker colors represent a higher stated discount factor (more patient) and higher income.

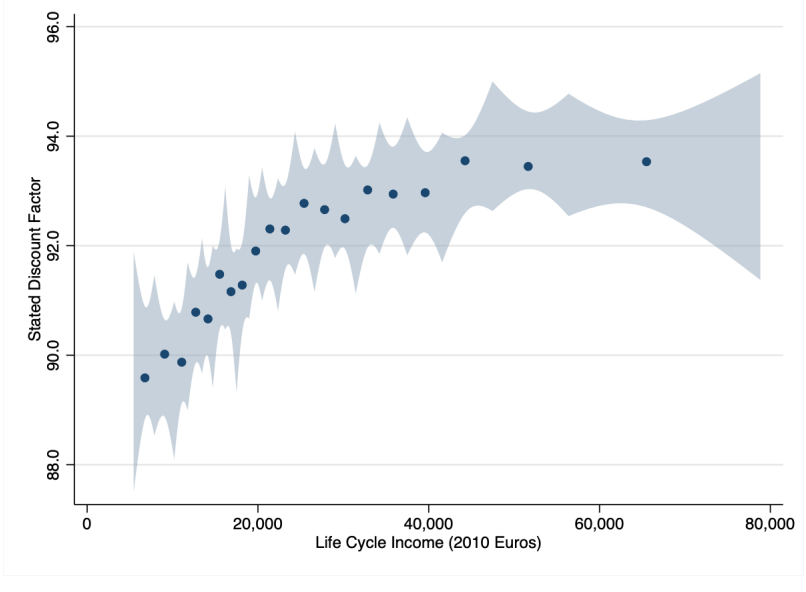


FIGURE 4.—Stated Discount Factor by Real Income Decile

Note: Binned scatter plot of the pre-earthquake stated discount factor, and life cycle income in 2010 euros.

3.4. *Identifying earthquake exposure*

We link SHIW households to seismic exposure at the municipality level. We classify a municipality as *damaged* if its maximum MMI is 6 or above, and as *shaken but not damaged* if its maximum MMI lies between 4 and 6. To mitigate concerns about endogenous relocation after the earthquake, households are linked to their municipality of residence as recorded prior to the event. For households first observed in 2010 or later, we use self-reported duration of residence to determine pre-earthquake location: if a household reports residing in a seismically affected municipality since before the earthquake, we classify it as exposed. Because SHIW interviews are conducted in person, self-reported residence provides a credible proxy for exposure. [19]

3.5. *Timing*

For consistency throughout the paper, we adopt the SHIW timing convention and refer to each wave by the year for which economic data are collected. However, it is important to note that these waves are always fielded in the following year. We observe the exact interview date, which is crucial for distinguishing whose stated time preference was measured before versus after the earthquake. The 2008 wave was fielded throughout 2009 (see Figure 5), with approximately half of the interviews conducted after the earthquake. Observing the exact interview date allows us to identify post-earthquake interviews. In contrast, income and consumption data in the 2008 wave refer to 2008, thus are pre-earthquake outcomes.

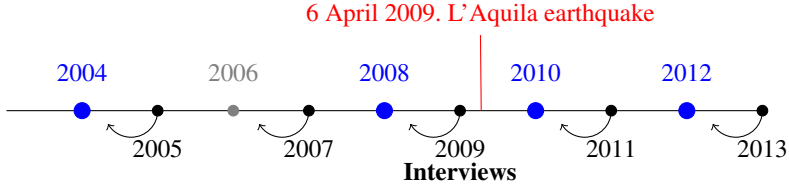


FIGURE 5.—Timing of SHIW Surveys.

Note: Interviews are conducted in years shown in black, eliciting previous year economic outcomes, i.e. interviews for the 2004 wave are conducted in 2005. Time preference questions were asked only in waves colored blue (2004, 2008, 2010, 2012).

3.6. *Estimation sample*

We observe time preferences for approximately 17,200 household heads across the 2004, 2008, 2010, and 2012 SHIW waves. Of these, approximately 5,000 households are observed both before and after the 2009 earthquake, allowing us to identify within-household variation in time preferences across the event.[20]

When examining savings and consumption behavior, we extend our panel sample to follow households over a longer period, from 2002 to 2016. We impose standard life-cycle restrictions following Deaton (1991), Attanasio and Weber (1995a) and Attanasio and Weber (1995b):

[19]For robustness, in Table VIII we restrict the sample to households with clear pre-shock presence, those interviewed in a shaken municipality shortly before the earthquake or those who lived in the same shaken municipality in both pre- and post-earthquake waves, to mitigate concerns about exposure misclassification. Our results remain robust.

[20]In Table VIII we also examine within individual variation of time preferences.

exclude households with a head younger than 25 years of age, or older than 70. These age groups are excluded because they make important decisions that are outside the scope of a simple life-cycle model, such as educational attainment for the young, decisions related to health shocks for the old (see Appendix A for details). These restrictions yield 4,645 households with observations both before and after the earthquake.

3.7. *Empirical Validation of the Identification Framework*

3.7.1. *Within-Individual Variation in Stated Preferences*

The cross-sectional correlation documented in Section 3.3 is consistent with stable individual heterogeneity rather than genuine bias. Identification requires the stronger condition: stated preferences respond to income changes within individuals over time. We validate this using the covariance structure implied by a permanent-income model. We model income as a random walk plus transitory shock, the standard decomposition in the consumption literature, because it yields testable predictions about the lead-lag structure of the income–patience covariance. We assume a linear version of the measurement equation (6).

$$\begin{aligned} \tilde{\beta}_{i,t} &= \alpha_0 + \alpha y_{i,t} + \tilde{\epsilon}_{i,t} \\ y_{i,t} &= \mu_{i,t} + \varepsilon_{i,t} \\ \mu_{i,t} &= \mu_{i,t-1} + \nu_{i,t}, \end{aligned} \tag{10}$$

where $y_{i,t}$ is log-income, composed of a random walk permanent component μ and an i.i.d. transitory component ε. The shock innovations $\nu_{i,t}$ and $\varepsilon_{i,t}$ are independent, have zero expected value and variances σ_ν^2 and σ_ε^2.[21]

Under these assumptions, the model yields a set of testable predictions regarding the covariance structure of stated preferences and income:

$$\begin{aligned} Cov(\tilde{\beta}_{i,t}, y_{i,t}) &= \alpha Var(y) \\ Cov(\tilde{\beta}_{i,t}, y_{i,t+1}) &= \alpha Var(\mu) < Cov(\tilde{\beta}_{i,t}, y_{i,t}) \\ Cov(\tilde{\beta}_{i,t}, \Delta y_{i,t+1}) &= -\alpha\sigma_\epsilon^2 \\ Cov(\tilde{\beta}_{i,t}, \Delta y_{i,t+2}) &= 0. \end{aligned} \tag{11}$$

Table I confirms all four moment conditions: the discount factor correlates more strongly with current than future income (columns 1–2), is negatively associated with next-period income growth (column 3), and statistically does not correlate with two-period-ahead growth (column 4), consistent with attenuation bias from transitory shocks.

3.7.2. *Pre-Earthquake Validation of Consumption-Savings Behavior*

To further validate the theoretical foundations of our identification framework, we examine consumption–savings behavior prior to the earthquake in Figure 6. Non-durable consumption rises with income but less than proportionally, yielding a declining consumption-to-income ratio, consistent with buffer-stock saving behavior (Figure 6(a)); less patient households consume a larger share of their income, consistent with standard consumption–savings theory (Figure 6(b)).

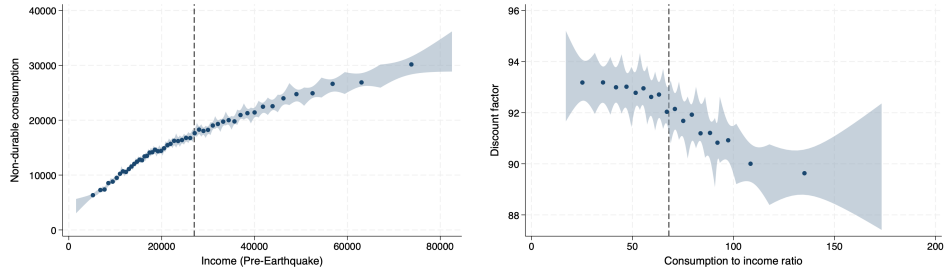
[21] For simplicity, in equations 10 we abstract from life-cycle characteristics $X_{i,t}$. In the empirical specifications controls include household head age, education, and liquidity proxies.

TABLE I

COVARIANCE OF STATED TIME PREFERENCE WITH INCOME

	Income Level		Income Change	
	(1)	(2)	(3)	(4)
	y_t	y_{t+1}	Δy_{t+1}	Δy_{t+2}
$\tilde{\beta}_{i,t}$	2.405	2.127	-0.673	-0.388
	(0.124)	(0.150)	(0.192)	(0.282)
Obervations	27,699	15,142	14978	10,651

Note: Note: The table reports estimated covariance of stated discount factors with income levels (columns 1 and 2) and income changes (columns 3 and 4). Standard errors are in parentheses.



(a) Consumption and Income (b) Consumption-to-income ratio

FIGURE 6.—Pre-Earthquake Consumption Patterns

Note: Binned scatter plots show pre-earthquake correlations: (a) non-durable consumption vs. income, (b) consumption-to-income ratio vs. the stated discount factor (time preference). Vertical lines mark mean income (€27,000) and mean consumption-to-income ratio (68%).

Together, these patterns confirm that the pre-earthquake data satisfy the behavioral regularities required for our identification framework. Having verified these prerequisites, we turn to the empirical design.

4. RESEARCH DESIGN

4.1. *Treatment Assignment: Proximity versus Experience*

Our goal is to identify preference shocks by exploiting exogenous exposure to the L'Aquila earthquake. A common approach in the literature is to proxy disaster experience by geographic proximity to the epicenter and estimate the following regression model on post-earthquake data:

$$\tilde{\beta}_{imt} = \alpha_1 1_{m \in Earthquake} + \mathbf{X}_{it}^{'} \alpha_X + \gamma_t + \epsilon_{imt}, \tag{12}$$

where $\tilde{\beta}_{imt}$ denotes the stated discount factor for household head i, residing in municipality m, and interviewed in year t. The indicator $1_{m \in Earthquake}$ captures alternative exposure definitions, vector $\mathbf{X}_{it}^{'}$ includes household and individual covariates (e.g., age, gender, education, household size, income, and liquidity). Year fixed effects γ_t absorb time-specific aggregate shocks. Standard errors are clustered at the municipality level, and all regressions use SHIW survey weights. This pooled OLS specification combines repeated cross-section and panel data

to maximize statistical power, and parallels common empirical designs relying on post-event elicitation only.

TABLE II

EFFECT OF EARTHQUAKE ON STATED DISCOUNT FACTOR - OLS (POST-EARTHQUAKE DATA)

	(1)	(2)	(3)	(4)
0 to 40 km	-3.891	-5.182		
	(1.268)	(0.726)		
40 to 125 km	0.122	0.615	0.122	
	(0.490)	(0.444)	(0.489)	
{40 to 125 km} X Shaken		-1.665		
		(0.385)		
Shaken (excl. $\leq$ 40 km)				-1.064
				(0.233)
Mean of dep. var.	91.6	91.7	91.6	91.7
Observations	15,973	15,526	15,842	15,413
R-squared	0.036	0.036	0.036	0.035

Note: OLS estimates of the 2009 Earthquake's impact on stated discount factors, using 2010 and 2012 data. The sample excludes household heads under 25 or over 90 at the time of the quake. Distance to epicenter used as intensity measure. Controls: age, education, income, credit constraint, wealth, and family size. Year fixed effects are included. Standard errors are clustered at municipality level; survey weights applied.

Column 1 of Table II reports results based on distance to the epicenter. The *inner ring* includes municipalities within 40 km that suffered substantial destruction. The *outer ring* covers 40–125 km, where seismic activity varied across locations. The control group, located more than 125 km away, experienced no ground motion. Households in the inner ring report, on average, 3.8 percentage point lower discount factors than those in further areas, a decline of approximately 4% relative to the pre-earthquake mean. This finding is consistent with prior evidence linking disaster proximity to changes in preferences (e.g., Cassar et al. (2017), Kuroishi and Sawada (2024)). In contrast, the outer ring is indistinguishable from the control group (see column 1 and 3). These differences may partly reflect the economic hardship and property losses concentrated in the inner ring, consistent with evidence that financial deprivation increases time discounting (e.g., Carvalho et al. (2016); Haushofer and Fehr (2014)).

However, treatment assignment based on distance alone masks substantial heterogeneity. Column 2 of Table II shows that those in shaken municipalities within the outer ring report discount factors 1.7 percentage points lower than those in unaffected areas. This highlights the importance of accounting for within-region variation in seismic exposure. Taken together, these findings motivate the construction of a finer-grained, experience-based treatment definition, *shaken but not devastated*, that isolates experiencing the earthquake from material destruction (column 4). Households in L'Aquila are therefore excluded, as widespread destruction likely induced displacement and economic disruption.

4.2. *Estimation Strategy*

We implement the identification framework outlined in Section 2 in two steps: (i) a difference-in-differences (DiD) model exploiting within-household changes over time, and (ii) an event study that traces the dynamic evolution of preference shocks and associated behavior. We compare households in municipalities that were *shaken but not devastated*, those that

experienced ground motion but little or no structural damage, to households in areas with no recorded seismic activity. By tracking the same households before and after the earthquake, we isolate within-household changes in stated preferences attributable to visceral exposure. Each approach offers complementary strengths: DiD controls for time-invariant household heterogeneity, and the event study tests timing and persistence of effects while assessing the parallel trends assumption.

The main outcome variables are stated time preferences, elicited through a hypothetical survey question on monetary trade-offs—commonly known as Money Earlier or Later (MEL), and observed consumption-savings behavior, which—through revealed preference theory—provides insight into individuals' underlying time preference.

4.2.1. *Difference-in-differences: exploiting household panel data*

To isolate changes in stated and revealed preferences attributable to earthquake exposure, we estimate a two-way fixed effects DiD model:

$$O_{imt} = \gamma_t + \gamma_i + \alpha_s 1_{m \in Shaken} + \alpha_1 1_{m \in Shaken} \times 1_{t \in After} + \mathbf{X}'_{it}\alpha_X + \epsilon_{imt}, \tag{13}$$

where O_{imt} denotes the outcome of interest (stated discount factor, consumption, or savings).[22] Year fixed effects γ_t capture time trends common to all households, and household fixed effects γ_i control for unobserved heterogeneity at the household level. The coefficient α_1 captures the average treatment effect on the treated (ATT) for households exposed to shaking compared to those that experienced no ground movements. The interaction term equals one for observations in shaken municipalities after the earthquake.

A key identifying assumption for DiD is that outcomes of the treatment and control groups would have evolved in parallel in absence of the earthquake, which we confirm in section 5.3. Parallel trends are plausible on two grounds.

First, seismic exposure was quasi-random. As discussed in Section 2, the earthquake generated two layers of exogenous variation: the epicenter's location, which determined regional exposure, and the local transmission of seismic intensity, which varied sharply even among equidistant municipalities. Some households experienced intense nighttime shaking; others nearby felt none. This setting approximates a natural experiment, in which ground motion was assigned with near-random precision across otherwise comparable households, enabling comparisons across households that are similar in observed characteristics but differ in seismic exposure.

Second, Table III shows that treated households (*shaken but not devastated*) and controls (*rest of Italy*) were broadly balanced on pre-earthquake observables, including age, income and the discount factor. While treated household are smaller and heads are more likely to have university degrees, these reflect slowly-evolving regional characteristics, for instance, the concentration of high-skilled employment in the broader commuting zone around Rome. Moreover, these differences are small in magnitude and unlikely to drive differences in post-treatment outcomes.[23]

[22] The timing conventions differs by variable: for stated time preferences, we classify post-earthquake status using interview dates; for consumption and savings, which refer to the prior calendar year, classification is based on survey wave (see Figure 5) In the 2008 wave, interviews were fielded throughout 2009, with some occurring after the earthquake. For these responses, we classify their time preference responses as post-earthquake but treat their consumption and savings as pre-earthquake outcomes, since these reflect end of 2008 conditions.

[23] Appendix Table VI shows that the observable characteristics among households living in municipalities within 40 km of the epicenter were, on average, similar to the *shaken but not devastated* group (column 1), but they have significantly lower pre-quake discount factors. This underscores a key limitation of post-event-only designs. Without pre-treatment measurements, underlying spatial variation in preferences may conflate the impacts of the treatment.

TABLE III
PRE-EARTHQUAKE DESCRIPTIVE STATISTICS

	Shaken (1)	*p-value* (2)	<125 km (3)	*p-value* (4)	Rest of Italy (5)
Head characteristics					
Stated discount factor (0-100)	91.80	0.502	92.21	0.797	91.94
Male	0.65	0.023	0.71	0.012	0.71
University degree (dummy)	0.18	<0.001	0.07	<0.001	0.09
Employed (dummy)	0.65	0.007	0.58	0.031	0.61
Household characteristics					
Annual household income	27,853	0.099	26,359	0.175	26,762
Annual savings to income ratio (0-1)	0.21	0.996	0.21	0.381	0.23
Wealth	256,746	0.013	212,235	0.019	221,066
Family size >2 (dummy)	0.37	<0.001	0.52	<0.001	0.49
Have children (dummy)	0.20	0.349	0.22	0.091	0.23
No. of households	511	-	1,874	-	18,248

Notes: The table reports the pre-earthquake average outcomes of individuals living in shaken (excluding L'Aquila) municipalities (column 1), those in unshaken municipalities within 125 km from the epicenter (column 3), and those in municipalities in the rest of Italy (column 3). p-value columns show the p-values of the hypotheses that the mean outcome of the group in the subsequent column is equal to the mean outcome of individuals in the shaken municipalities. The pre-earthquake data on the stated discount factor are from the 2004 and 2008 waves of the SHIW (in 2008, only responses from individuals interviewed before the 2009 Earthquake are used). Pre-earthquake data on other variables come from the 2002, 2004, 2006 and 2008 waves. Monetary values are in 2010 euros. Survey weights are used.

A potential threat to the DiD design arises if attrition is systematically related to treatment status. Differential dropout rates or differing determinants of attrition across groups can alter the composition of the surviving sample, violating parallel trends. In disaster settings, the most adversely affected households are often most likely to relocate. In our context, however, treatment refers to exposure to seismic motion rather than to physical destruction, so relocation incentives are substantially weaker. Indeed, only 1.6% of SHIW households moved to a different municipality after the earthquake, and Appendix Table VII confirms that attrition is not selective.

4.2.2. *Event study: dynamics and duration of preference shocks*

To test the timing and persistence of effects, and to validate the parallel trends assumption, we estimate an event study specification:

$$O_{imt} = \gamma_t + \gamma_i + \alpha_s 1_{m \in Shaken} + \sum_{t \neq 2008} \alpha_t 1_{m \in Shaken} \times 1_t + X'_{imt} \alpha_X + \epsilon_{imt}. \quad (14)$$

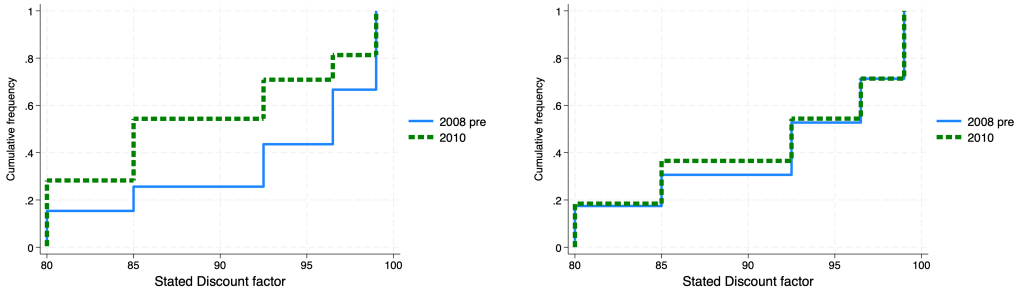
The coefficients of interest are α_t, which trace the treatment effect in each year relative to the base year 2008. For stated preferences, we further restrict the reference group to respondents interviewed before the earthquake in the 2008 wave. While stated preferences are available in only two post-earthquake waves (2010 and 2012), we observe consumption and savings for a longer window. If preference shocks are persistent, they should manifest as durable behavioral shifts in financial outcomes.

5. EMPIRICAL RESULTS

5.1. *Stated Time Preferences*

5.1.1. *Distributional Evidence*

Figure 7 presents empirical cumulative distribution functions of stated time preferences for the Shaken and control groups, separately before and after the earthquake. Responses are recorded in discrete bins. After the earthquake, the entire distribution of the shaken group shifted leftward, indicating that the decline in patience was not concentrated among a subgroup but was pervasive across the sample.[24]



(a) Shaken Treatment (b) Control

FIGURE 7.—Stated Discount Factor Before and After the Earthquake. *Notes*: The figure plots the cumulative distribution of stated discount factor for individuals living in shaken and control municipalities.

5.1.2. *Difference-in-Differences Estimates*

Table IV addresses three potential confounds in sequence: omitted time-invariant heterogeneity, time-varying economic conditions, and financial constraints. Columns 1-2 provide a cross-sectional OLS benchmark using only post-earthquake observations (12), establishing that shaken households report lower patience than unshaken ones, even after controlling for demographic and financial differences. Because the OLS design lacks pre-earthquake measurements, it cannot account for pre-existing spatial differences in baseline patience; columns 3-8 address this by introducing the DiD structure (13), restricting identification to within-household changes over time. We include household and municipality fixed effects to absorb all time-invariant differences, including stable regional characteristics and permanent income. Notably, the DiD estimates exceed the OLS estimates, implying that studies eliciting stated preferences only post-disaster–the typical research design–would understate the true change, as unobserved heterogeneity partially offsets the treatment effect. The treatment effect is stable across columns 3 and 4, before and after adding demographic controls, confirming that shifts in household composition do not drive the result. Columns 5-7 then sequentially add liquid assets, wealth, and credit constraint proxies. The stability of the coefficient across these columns addresses the concern that the preference shift is an artifact of changing financial conditions:

[24]Prior to the earthquake, the distributions of stated time preferences were statistically indistinguishable across treatment and control groups ($\chi^2(4) = 1.10$, $p = 0.89$). Following the earthquake, the distributions are significantly different ($\chi^2(4) = 28.67$, $p < 0.001$).

if tightened liquidity were the true mechanism, controlling for it directly would attenuate the estimate, but the coefficient remains unchanged across all three columns. Column 8 additionally controls for HTM status, following Kaplan et al. (2014), which is the most direct test of the constraint channel; the estimate again remains unchanged. Taken together, the progression across columns demonstrates that the decline in stated patience is not explained by pre-existing group differences, demographic change, income shocks, wealth effects, credit frictions, or liquidity constraints, but instead reflects a direct behavioral response to earthquake exposure. In Section 5.3, we provide direct evidence supporting the parallel trends assumption underlying this strategy using pre-trend estimates from an event study.

Panel A uses the national unshaken sample as the control group. The results consistently show a decline of approximately 5 percentage points in the stated discount factor for the Shaken group, equivalent to a shift from the median to approximately the 25th percentile of the national distribution.

5.1.3. *Spatial Control*

A remaining concern is that households near L'Aquila may face local spillovers, disrupted labor markets, price shocks, or social obligations that the national control in Panel A cannot absorb. Panel B addresses this directly, restricting comparison to a spatial control group: households in unshaken municipalities located within 125 kilometers of the epicenter, the maximum range of recorded ground motion. This design exploits the fact that both treated and control households reside at comparable distances from the epicenter, yielding two identification advantages.

Proximity enables quasi-random assignment: conditional on the epicenter's location, the nature of seismic propagation (see Section 3) implies that treatment assignment is as good as random across similarly situated municipalities. Hence, nearby unshaken areas provide plausible counterfactuals where shaking could have occurred but did not.

Proximity to L'Aquila, a regional economic and administrative hub, also implies shared exposure to general equilibrium effects. The earthquake's destruction induced widespread disruptions in the region, including business closures, and displacement. In 2009, Abruzzo's GDP contracted by 6%, far exceeding the national average, and the region experienced disproportionately large employment losses. We confirm with a difference-in-differences event study (Appendix Figure 12) that indeed disposable household income trajectories for the shaken and spatial control groups remained parallel over time, reinforcing the plausibility of shared indirect exposure.[25]

This design departs from the standard Stable Unit Treatment Value Assumption (SUTVA) by explicitly accommodating spatial interference. Rather than assuming no spillovers across treated and non-treated, we construct a control group plausibly subject to similar general equilibrium forces as the treated households, allowing us to net out these effects and isolate the direct behavioral effects of earthquake-induced shaking. This logic follows the spatial inference approach proposed by Bernheim et al. (2021), wherein proximity is leveraged to mitigate local interference, allowing us to net out these effects and isolate the direct behavioral effects of earthquake-induced shaking

[25] We further probe for indirect spillovers using SHIW microdata. No differential change is observed in financial transfers to relatives or friends. Unemployment per household rose by 0.4 in both groups. Spending on extraordinary home maintenance remained flat, and property-backed loans rose modestly and symmetrically (from 2.0 to 2.3 loans on average per 100 households). We find no evidence of households giving shelter to relatives; the average household size (excluding births) declined by 0.2 in both groups. There is some evidence of increased financial transfers to relatives or friends, but there is no differential change in the two groups. While based on limited responses, these indicators provide no evidence of systematic indirect spillovers.

TABLE IV

STATED TIME PREFERENCE

	OLS		DiD					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Rest of Italy								
Shaken x After			-5.413	-5.536	-5.536	-5.535	-5.531	-5.501
			(0.596)	(0.604)	(0.605)	(0.604)	(0.605)	(0.605)
Shaken	-1.041	-1.232						
	(0.343)	(0.329)						
Mean of dep. var.	91.5	91.7	91.9	92.1	92.1	92.1	92.1	92.1
Observations	16,279	15,526	14,413	13,688	13,688	13,688	13,688	13,688
R-squared	0.034	0.032	0.520	0.522	0.522	0.522	0.522	0.522
Panel B: Spatial Control								
Shaken x After			-6.061	-6.403	-6.121	-6.411	-6.384	-6.415
			(0.848)	(0.892)	(0.927)	(0.891)	(0.889)	(0.890)
Shaken	-1.727	-1.798						
	(0.459)	(0.450)						
Mean of dep. var.	91.7	91.8	91.9	91.9	91.9	91.9	91.9	91.9
Observations	2,117	2,040	1,517	1,464	1,464	1,464	1,464	1,464
R-squared	0.055	0.056	0.556	0.566	0.569	0.566	0.569	0.567
Income	yes	yes	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	yes	yes	yes	yes
Liquid asset	no	no	no	no	yes	no	no	no
Wealth	no	no	no	no	no	yes	yes	yes
Credit constraint	no	no	no	no	no	no	yes	no
HTM	no	no	no	no	no	no	no	yes

Note: The dependent variable is the stated discount factor (0–100), capturing time preferences. Columns 1–2 present OLS estimates using post-earthquake data. Columns 3–8 report DiD estimates of the 2009 Earthquake's impact. Data come from the 2004, 2008, 2010, and 2012 SHIW waves. The sample excludes households with heads under 25 or over 90 at the time of the Earthquake. OLS models include year fixed effects; DiD models include household, initial municipality and year fixed effects. Controls: household head's age and education, family size, and presence of children. Standard errors clustered at the initial municipality level (in parentheses).

Estimated treatment effects reported in Panel B are stable in magnitude and slightly larger than those obtained with the national control sample, consistent with the declining income in the area partially mediating the treatment effect on stated patience.

5.1.4. *Robustness and Alternative Explanations*

Table VIII assesses sensitivity to sample composition, measurement, and other confounding shocks. The most important check concerns within-household respondent variation: if the identity of the survey respondent changes across waves, household fixed effects may conflate genuine preference shifts with compositional changes. However, results are unchanged when using respondent rather than household fixed effects (columns 1 and 5), confirming that the estimate reflects individual-level changes rather than household turnover. Excluding Rome, which is atypical in size and administrative status, leaves results unchanged (columns 2 and 6), as does dropping households subsequently affected by the 2012 Emilia-Romagna earthquake (column 3). Re-estimating with an interval-based coding of the MEL instrument, which accounts for

changes in response options across waves, yields similar results (columns 4 and 7). Finally, Appendix B provides evidence that attrition is not selective by treatment status.

5.1.5. *Curvature of the utility function*

Our baseline measure assumes risk neutrality, so MEL responses map directly to discount factors. Under curvature of the utility function, greater risk aversion inflates the willingness to accept immediate payment, biasing the discount factor downward irrespective of true patience (Andersen et al. (2008)). This bias cancels in first differences if risk preferences are stable, but disasters can shift risk attitudes (see for example Hanaoka et al. 2018, Cheong 2022, Quiroga and Tanner 2024), introducing a post-earthquake downward bias that would cause us to overstate the decline in patience.

We assess this threat using a recurring investment-attitude question elicited since 2004 (Appendix A). Appendix Figure 15 shows that treated households became more risk-averse in the short-run after the earthquake, confirming that the concern is empirically relevant. We therefore re-estimate treatment effects after adjusting for utility curvature. Because the investment-attitude question may partly reflect beliefs about returns rather than pure risk aversion, we anchor the adjustment using an incentive-compatible lottery measure of the Arrow–Pratt coefficient elicited in 2010, which we then impute to all waves via the estimated relationship between the two measures (Appendix C.2). Curvature-adjusted estimates (Table XI) remain statistically significant, though smaller in magnitude, indicating that the shift in risk preferences cannot account for the observed decline in stated patience.

These findings establish the first step of our identification framework: stated patience declined sharply and persistently among exposed households. We now examine whether consumption behavior moved in the direction predicted by a genuine preference shock.

5.2. *Effects on Consumption*

Table V tests the consumption prediction directly using annual non-durable consumption data. We restrict the sample to non-HTM households aged 25-70, following the criteria in Section 2.4.[26] We focus on the short run, the 2010 and 2012 waves, because sustained consumption financed through asset drawdown may attenuate the treatment coefficient once liquid buffers are exhausted; we examine the longer horizon in Section 5.3.

Across all specifications, from the post-earthquake OLS benchmark in columns 1–2 to the DiD estimates with household and municipality fixed effects in columns 3–7, the treatment effect is stable and robust to sequential inclusion of liquid assets, wealth, and credit constraint controls. This rules out the possibility that the consumption increase reflects changing financial conditions rather than preference shifts. Panel B confirms similar results against the spatial control group, which exhibited parallel income trajectories (Figure 12), ruling out local income dynamics as an alternative explanation.

The estimated magnitude of €2,100 increase in annual non-durable consumption, approximately 11 percent of the pre-treatment mean - is economically substantial.[27] Two benchmarks help gauge the magnitude. First, compared to the iMPC from large hypothetical windfalls estimated on Italian data by Jappelli et al. (2026), our effect persists well beyond the horizon at which windfall-driven consumption increases typically dissipate, consistent with a preference shift rather than a transitory liquidity response. Second, using pre-earthquake SHIW data,

[26] HTM households constitute approximately 28 percent of the sample.

[27] This corresponds to €175 per month, roughly the cost of one restaurant dinner for two per week.

TABLE V

SHORT-RUN EFFECTS ON ANNUAL NON-DURABLE CONSUMPTION - NON HAND-TO-MOUTH HOUSEHOLDS

	OLS		DiD				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Rest of Italy							
Shaken x After			2108.2	2121.8	2139.9	2128.8	2107.3
			(405.9)	(406.4)	(399.9)	(407.0)	(402.2)
Shaken	1189.5	1171.0					
	(430.0)	(411.1)					
Mean of dep. var.	19,541	19,553	19,149	19,165	19,165	19,165	19,165
Observations	3,372	3,360	11,590	115,37	11,537	11,537	11,537
R-squared	0.438	0.460	0.728	0.730	0.730	0.730	0.730
Panel B: Spatial Control							
Shaken x After			2302.0	2361.2	1920.6	2635.2	2643.6
			(993.8)	(1142.8)	(1031.4)	(992.4)	(991.5)
Shaken	-337.6	-278.2					
	(778.7)	(929.1)					
Mean of dep. var.	20,970	20,970	19,658	19,667	196,67	19,667	19,667
Observations	342	342	1,277	1,271	1,271	1,271	1,271
R-squared	0.468	0.501	0.712	0.714	0.717	0.717	0.717
Income	yes	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	yes	yes	yes
Liquid asset	no	no	no	no	yes	no	no
Wealth	no	no	no	no	no	yes	yes
Credit constraint	no	no	no	no	no	no	yes

Note: The dependent variable is annual non-durable consumption (2010 euros), excluding imputed rent. Columns 1–2 show OLS estimates using post-earthquake data. Columns 3–7 report DiD estimates of the 2009 Earthquake's impact. Data come from the 2002–2012 SHIW waves, capturing short-run effects (within 3 years of the Earthquake). The sample excludes households with heads younger than 25 or older than 90 at the time of the Earthquake and is further restricted to respondents aged 25–70 at the time of the survey and those who are non-HTM in all pre-quake waves. OLS models include year fixed effects; DiD models include household and year fixed effects. Controls include household head's age, education, family size, and presence of children. Standard errors clustered at initial municipality level (in parentheses).

we estimate an MPC of 37 percent by instrumenting income with job loss shocks, following Patterson (2023) and Bilbiie et al. (2025) (see Appendix F for details). In the first year of unemployment, the average household loses roughly €830 per month, implying a monthly consumption decline of approximately €300 (Appendix Figure 18). The earthquake-induced increase of €175 per month is of comparable order, suggesting a reallocation of similar scale to a large transitory income shock, but in the opposite direction, and driven by preference rather than constraint.

The joint movement of stated patience and consumption in the directions predicted by a genuine preference shock completes the second leg of our identification. We now examine whether these effects persisted.

5.3. *Dynamics*

We now examine the dynamics of these effects. The event study design allows us to assess how rapidly the preference shift emerged and whether it persisted long enough to generate durable reallocations in savings and portfolio holdings.

5.3.1. *Immediate and Persistent Changes in Stated Preferences*

Figure 8 presents event study estimates of stated time preferences using specification 14. All post-event coefficients are interpreted relative to respondents interviewed prior to the April 6, 2009 in the 2008 wave. We separately isolate individuals interviewed later in 2009 to estimate the immediate effect.

Stated patience fell sharply and immediately: respondents interviewed within weeks of the earthquake already report significantly lower discount factors, with no evidence of recovery through 2012. These patterns hold with the spatial control group, ruling out regional economic spillovers. The flat pretrends visible in Figure 8 validate the parallel trends assumption. If this

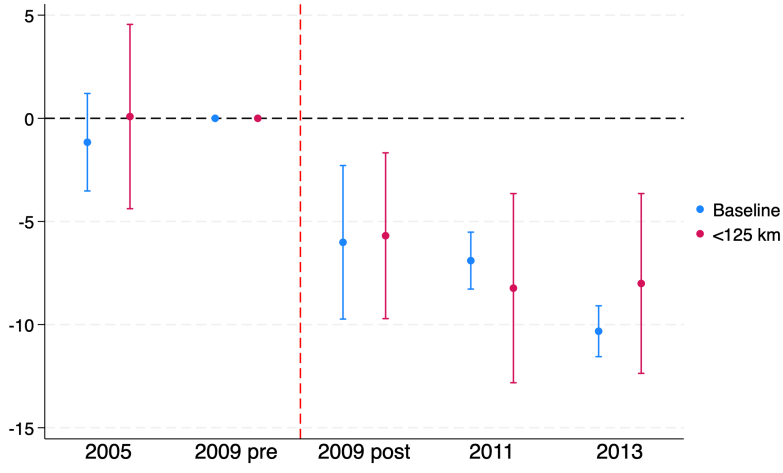


FIGURE 8.—Effects on Stated Discount Factor - Event Study.

Note: The figure plots the event study estimates of estimating equation 14 using the TWFE estimator on the stated discount factor. The y-axis shows the estimated effects of the 2009 L'Aquila earthquake. In this framework, the reference periods are the 2009 interview dates before 6 April, 2009 in the SHIW 2008 wave. The baseline estimates include all individuals in the analysis sample and does not control for risk aversion. The blue symbols and lines represent a specification using unshaken households in the rest of Italy as the control group; and the red symbols and line represent estimates using individuals from within 125 km of the epicenter as control.

decline reflects a genuine preference shift rather than a transitory response to trauma, it should generate a durable reallocation in consumption and savings, not a level effect that reverts as the shock recedes.

5.3.2. *Persistent Effects on Consumption and Savings*

We extend the analysis to seven years post-earthquake, four years beyond the last stated-preference observation, to test whether behavioral shifts are as persistent as the preference decline.

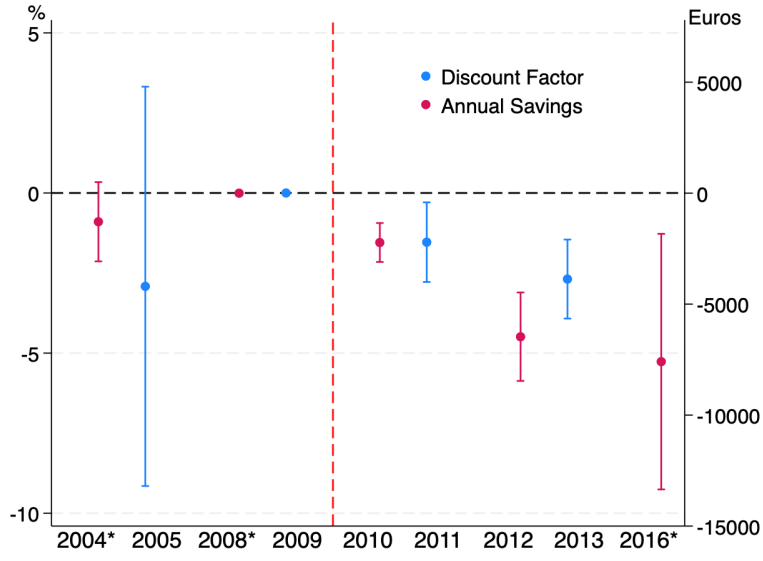


FIGURE 9.—Long Term Effects on Savings and Stated Patience (Unconstrained Households) - Event Study.

Note: The figure plots event study estimates of the effects of earthquake exposure on the stated discount factor and annual savings. The sample includes households classified as non-HTM in all periods with data available prior to the earthquake, with heads aged 25–70 at the time of the survey. Due to limited sample sizes in early and late SHIW waves, we pool 2002–2004, 2006–2008, and 2014–2016 for estimation on savings (red). We show in Figure 14 that the event study estimates without pooled years are consistent. For results on consumption, see Figure 13.
* indicates pooled years.

Among working-age, non-HTM households prior to the earthquake savings fell persistently following the earthquake (Figure 9), with confidence intervals widening in later years as panel attrition reduces sample size.[28] Consumption shows the mirror pattern: Figure 13 documents a persistent increase in non-durable spending that tracks the savings decline.[29] The consumption effect itself attenuates over the longer horizon, consistent with the budget constraint eventually binding: a permanent upward shift in the discount rate raises the desired consumption path but cannot indefinitely outpace income.

Elevated risk aversion, had it been the driver, would have raised precautionary savings and dampened consumption, the opposite of what we observe. Since risk aversion returned to baseline within two waves (Appendix Figure 15), it cannot account for either the direction or the persistence of the behavioral shift. Taken together, the persistent reduction in savings among households with the financial means to maintain pre-shock plans is consistent only with a lasting shift in time preferences.

5.3.3. *Financing the Consumption Increase: Portfolio Adjustments*

The sustained consumption increase raises a natural question about balance sheets. If the preference shift is genuine and persistent, liquid assets should decline and debt accumulate as households finance elevated spending over time. We examine this by following Georgarakos et al. (2024) and Coibion et al. (2024) and estimating:

[28]To improve precision at the panel's tails, we pool observations biannually, except for 2010 and 2012.
[29]Durable consumption also rises in the short run but attenuates by 2016; see Appendix Figure 13.

$$F_{imt} = \gamma_t + \gamma_m + \alpha_1 \Delta\tilde{\beta}_{it}^{Post} + \alpha_2 \tilde{\beta}_{it}^{Pre} + \mathbf{X}'_{it}\alpha_X + \epsilon_{imt}, \tag{15}$$

where F_{imt} denotes the household's rank in the distribution of liquid assets, broader financial holdings, or bank liabilities. We use the 2014 and 2016 waves to capture portfolio adjustments five to seven years after the earthquake.[30]

The results in Appendix Table X show a clear pattern of dis-saving. Households with a larger post-earthquake decline in patience hold lower financial assets and carry higher bank liabilities, consistent with sustained consumption financed through drawdown and borrowing. Controlling for pre-earthquake patience levels leaves the estimates unchanged, confirming that preference changes, rather than stable individual heterogeneity, drive the portfolio reallocation in the long run. By 2014-16, treated households moved down nine percentile points in the national liquid wealth distribution.

Together, the evidence on persistent stated impatience, durable consumption increases, and deteriorating balance sheets establishes that the earthquake generated a lasting shift in time preferences with material consequences for household financial resilience. We now ask what mechanism can account for effects of this magnitude and duration.

6. MECHANISMS

How can experiencing an earthquake make individuals more present-focused? We first show that neither increased uncertainty nor pessimistic survival beliefs can fully account for the results. We then argue that psychological channels are also plausible and provide heterogeneity evidence consistent with this view.

6.1. *Uncertainty*

Severe disasters raise both objective and perceived uncertainty (Guiso et al. (2008), Baker et al. (2024), Gallagher (2014)). Our identification using space should net out objective uncertainty of income or employment because treated and spatial controls are at a similar distance from the epicenter, and their realized income dynamics were also moving in parallel (see Figure 12).

Subjective uncertainty may have risen more in the treated group as in Gallagher (2014), because the experience was more salient to them. Yet, in standard models, greater uncertainty raises precautionary saving and lowers current consumption (see buffer stock saving model Carroll (1997)), the opposite of what we observe. Thus, increased uncertainty would attenuate, not amplify, the apparent impatience shock.

6.2. *Beliefs about survival, and disaster risk*

Another potential mechanism involves beliefs about survival or the likelihood of future disasters. Both affect the perceived value of the future. Life-cycle models formalize survival probability with a"mortality-adjusted" discount factor:

$$u'(C_{i,t}) = \delta_{i,t}\beta_{i,t}E_t(1+r)u'(C_{i,t+1}), \tag{16}$$

[30] $\Delta\tilde{\beta}_{it}^{Post}$ is computed from the same households used to estimate the DiD. Stated discount factor is a noisy measure of the true underlying discount factor; to the extent that measurement error is classical, the portfolio coefficients are attenuated toward zero; and the estimates in Table X are therefore conservative.

where $\delta_{i,t}$ denotes the subjective survival probability. Lower δ mimics a fall in β, but implies reduced life expectancy.

While time discounting and survival beliefs are observationally equivalent in this formulation, we can still assess plausibility by examining whether the implied change in life expectancy required to match our estimates is reasonable. Following Falk et al. (2019), stated patience increases with expected longevity. Using ISTAT life tables, a 40-year-old in 2010 had a remaining life expectancy of 42 years. We calculate that for a 40 year old, interpreting our estimated 5 percentage points decline in mortality-adjusted discount factor purely through survival probability δ, would be equivalent to halving expected lifetime, an implausible magnitude.[31] We conclude that longevity beliefs can explain at most a small share of the effect.

A related, belief-based argument was proposed in Gallagher (2014): disaster probability beliefs increased after U.S. floods, leading to an increase in insurance uptake. A similar effect could contribute to lower savings in our setup. Yet, we find no post-quake change in home-insurance rates among shaken households. Coverage remains low ($\approx 12\ \%$) and increases only marginally over time. This suggests that perceived disaster risk did not rise enough to drive the observed behavioral shift.[32]

6.3. *Psychological channels of mortality awareness*

Having ruled out changes in survival beliefs as the primary mechanism, we turn to psychological channels associated with mortality awareness. A large body of evidence, surveyed by Haushofer and Fehr (2014), shows that exposure to stress increases present focus. Experimental studies find that affective priming (e.g., stressful videos; Lerner et al. (2013)), physical stimuli (e.g., electric shocks), and pharmacological interventions (e.g., hydrocortisone) all lower patience (Ifcher and Zarghamee (2011)). These effects are stronger when stress is salient or repeatedly triggered.[33]

In the L'Aquila case, psychological distress and stress-related sleep disorders were widely reported in medical studies, both the city and surrounding areas, and persisted for years (Tempesta et al. (2013a); Rossi et al. (2011)). The earthquake struck in the middle of the night, abruptly waking residents. Beyond the initial trauma, stress was reinforced by recurring reminders: the trial and conviction of public officials and geologists, a reconstruction effort marred by delays and corruption, and the visible scars of destruction. These events sustained media and public attention well beyond the disaster.[34] During our 2019 visit, ten years after the earthquake, tensions remained palpable. Residents used a keynote lecture at a public conference to confront local authorities over reconstruction delays.[35] These reminders likely sustained the salience of

[31] This magnitude also exceeds the estimates in Falk et al. (2019), who find that a 10-year increase in life expectancy corresponds to a five percentage point increase in discount factors.

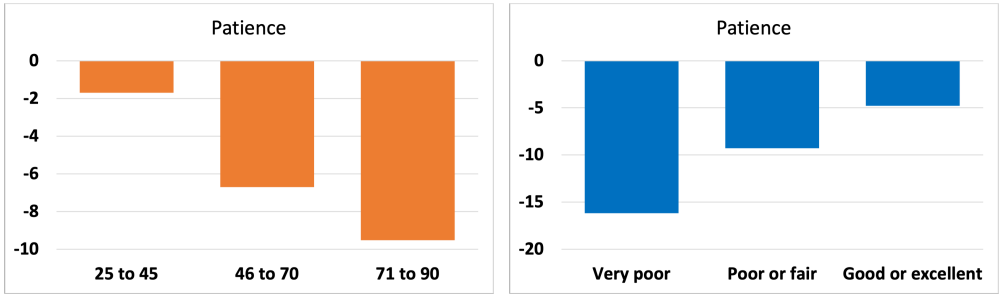
[32] One reason behind the low housing insurance coverage among shaken households might be moral hazard, as the state often rebuilds private homes. Yet, there would still be value in private insurance, because, as we observed during our visit to L'Aquila, the speed of rebuilding was much faster with private insurance.

[33] Salience refers to the phenomenon where attention is disproportionately directed toward certain aspects of the environment (Taylor and Thompson (1982)). Building on this insight, Bordalo et al. (2012) develop a psychologically grounded model of attention allocation, showing how shifts in salience can lead to systematic instability in risk preferences

[34] Geologists who served on the official risk advisory committee were prosecuted and imprisoned, drawing sustained national and international media coverage for over seven years (Cartlidge (2016)).

[35] At the Silk Cities Conference held in L'Aquila on July 12, 2019, residents voiced frustration that, ten years after the earthquake, children were still studying in temporary buildings. Though not formally invited, they attended a keynote by the Department of Civil Protection to protest.

the event and exacerbated cognitive and emotional strain, consistent with psychological research showing that chronic environmental cues amplify the impact of trauma (Lazarus and Folkman (1984)).



(a) Patience change by age group (b) Patience change by subjective health

FIGURE 10.—Heterogeneous Preference Changes by Age and Health. *Notes*: The figure plots the coefficients of the interaction terms between the treatment indicator *Shaken* × *After* and an indicator variable for each of the three age or self-assessed health bins at the time of the L'Aquila earthquake. Estimation results are reported in Table XII.

If psychological distress drives the change in time preferences, effects should be stronger among groups more vulnerable to stress. Medical studies show that older individuals were more likely to suffer sleep disturbances and were prescribed more antidepressants and antipsychotic medication (Tempesta et al. (2013b); Rossi et al. (2011)). Figure 10(a) shows a clear age gradient: the largest declines in patience are found among those aged 71 and above. Preexisting poor health also predicts stronger effects. Using a self-assessed health measure, Figure 10(b) confirms that the individuals in poor health experienced stronger effects. Since poorer baseline health amplifies vulnerability to stress, the stronger effects among the elderly are consistent with health-mediated channels. Quantitatively, the earthquake's effect on self-assessed health is comparable to the decline associated with aging by about ten years (Table XIII).[36] In the Online Appendix (Figure 17), we explore complementary mechanisms relating to stress, including the scarcity of economic and cognitive resources.[37]

7. CONCLUSION

This paper develops a new identification framework to estimate time preference shocks, using joint variation in stated and revealed preferences. The approach leverages directional predictions from a standard consumption–savings model: while both income shocks and impatience shocks lower stated patience, they imply opposite movements in consumption. By combining hypothetical responses with observed behavior before and after a salient shock, we identify

[36]A growing literature documents persistent adverse health effects of natural disasters. Following the 2011 Christchurch earthquake, Fergusson et al. (2014) report increased incidence of anxiety, sleep disorders, and cardiovascular risk. Similar long-run consequences are found after Hurricane Katrina (Raker et al. (2020)), the Chernobyl disaster (Danzer and Danzer (2016)), and in meta-analyses of post-disaster trauma (Heanoy and Brown (2024)).

[37]Economic factors are shown as stressors causing impatience in Carvalho et al. (2016). The role of cognitive resources for time preferences is shown in Dohmen et al. (2010) and in theoretical models like Gabaix and Laibson (2021), which argue that higher discounting can result from limited cognitive resources, which can deteriorate after a negative health shock.

changes in latent discount factors even when stated preferences are biased by economic constraints.

We apply this strategy to households affected by the 2009 L'Aquila earthquake and find persistent reduction in patience. Treated households raise consumption, draw down liquid savings, and accumulate less wealth in subsequent years, a pattern consistent with a long-lasting decline in patience. These effects are concentrated among older and less healthy individuals, supporting a stress-related interpretation of the underlying mechanism.

If traumatic events such as natural disasters become more frequent or widespread, e.g., under climate change, shifts in time preferences may have broader macroeconomic implications. In particular, preference shocks may reduce the willingness of affected cohorts to save, weakening the scope for intergenerational risk-sharing. In overlapping-generations economies with incomplete markets, such behavioral responses may limit the effectiveness of public insurance and shift more risk onto future generations.

More generally, widespread changes in time preferences could lower aggregate savings rates, amplify the transmission of shocks, and complicate stabilization policy. Incorporating preference shocks into macroeconomic models of resilience may be essential for understanding long-run economic adaptation under increasing climate risk.

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APPENDIX A: DATA

A.1. *Sample Restrictions*

Our estimation sample on time preferences consists of households with heads who were between 25 to 90 years of age at the time the earthquake hit in 2009. To ensure that our estimates are based on average Italians, we further restrict our sample in the following steps. First, we exclude households with fewer than 10 of life-cycle income per year. We also remove households who reported consuming more than three times it's life-cycle income in any given year. We also trim the top and bottom 1 percent of the sample in terms of key outcome variables, including life cycle income, consumption of durables, non-durables, monthly spending, savings, bank liabilities and outstanding loan. If a household has at least one of these variables in the top or bottom 1 percent of the distribution, we remove the household from the analysis.

A.2. *Discount factor*

The SHIW elicits time preferences from household heads in four waves (2004, 2008, 2010, and 2012) using the following money-now or money-later style question:

> Imagine that you find yourself in the following situation. You have won the lottery and will receive a sum equal to your household's net yearly revenue. You will receive the money in a year's time. However, if you give up part of the sum you can collect the rest of your win immediately.

where respondents are asked, in a stepwise manner, the % of the hypothetical win that they are willing to give up to receive the rest of the lump sum immediately. To avoid inconsistent answers, if someone stated that she was willing to give up 2% of the lottery win but unwilling to give up 5%, then she was not asked further about other cutoff values. This elicitation method allows us to determine the interval in which the 'true' cutoff value lies, which is $2-5\%$ in this example.[38]

As our main outcome of interest, we assume risk neutrality and assign to each household head one minus the midpoint of this interval as his discount factor. In the same example, the discount factor would be 96.5% as the midpoint is 3.5%.[39] In general, the curvature of the utility function can impact stated time preference.[40] A more risk-averse person might be more willing to choose an immediate and certain income rather than a future income, which is uncertain in real terms. We show in Table XI that our results are consistent when using an alternative time preference measure without assuming risk neutrality.

A.3. *Risk Aversion*

We also focus on a second outcome of interest, risk preferences. To measure this outcome, we use information collected from household heads from the following investment attitude question:

> In managing your financial investments, would you say you have a preference for investments that offer: (1)very high returns, but with a high risk of losing part of the capital, (2)a good return, but also a fair degree of protection for the invested capital, (3) a fair return, with a good degree of protection for the invested capital, (4) low returns, with no risk of losing the invested capital.

where respondents are asked this question in every survey wave since 2004.[41] In addition, in 2010, an Arrow-Pratt measure of risk aversion was elicited, using a lottery question. We use this 2010 measure to impute a cardinal risk aversion measure (see Appendix C) to show that our results are robust to using either measure.

A.4. *Consumption and Income*

To measure life-cycle income, we follow the literature to add payroll income (including net wages and salaries, and fringe benefits), pensions and net transfers, and net self-employment income (see e.g., Blundell et al. (2008)). For consumption, we derive two variables. First, non-durable consumption is the sum of all expenditure categories, including food and non-food, except durable goods. Importantly, we exclude imputed rents from this variable. Second, durable consumption includes all expenditures on durable goods, including transport equipment, furniture, and household appliances. Finally, we subtract the non-durable and durable consumption expenditures from life-cycle income to arrive at our measure of life-cycle savings. All monetary variables are deflated to 2005 euros.

[38] An advantage of the simplicity of this survey question is the high response rate, which is 88% on average. Recent experimental measures elicit time preference in more complex ways, asking similar questions across different time horizons, and eliciting quantitative risk preference measures at the same time (see Andersen et al. (2008)).

[39] We show that our estimation is robust to using the entire interval in Table VIII.

[40] See Keller and Strazzera (2002) and Frederick et al. (2002) for a discussion.

[41] In 2004, only half of the population was asked this question.

APPENDIX B: SELECTIVE NON-RESPONSE AND ATTRITION

In this section, we provide supportive evidence that selective non-response to the patience question and sample attrition after the earthquake are generally not a concern in the SHIW and in our main specifications, which use individual fixed effects.

First, Table VII reports the averages of key characteristics before the earthquake. Among those who are shaken, households that stayed in the SHIW after the earthquake and those who left are not statistically different across all relevant observables, except for the number of working family members (significant at the 10% level). Households that stayed had 1.7 worker household members relative to 1.5 among the attrited group. Importantly, the stayers and those who left were no different in their elicited discount factor in survey years prior to the earthquake. Within the rest of Italy, the large control group, stayers and leavers had similar levels of elicited patience on average. There are, however, statistically significant differences in various observables, including household income (stayers were richer), savings to income ratio (stayers saved more), family size (stayers had larger families), number of working household members (stayers had more workers), and children (stayers were more likely to have children). Overall, we are not overwhelmingly concerned about these differences for two reasons. First, given our main result that shaken households became more impatient and saved more after the earthquake, it means that if the leavers had stayed in the SHIW, our results would be even stronger, as the control group would have lower savings and lower income. Second, Table VII shows that within the unshaken households that lived within 100 km from the epicenter, the small control group, there are no longer statistically significant differences in these observables between the stayers and movers.

Second, we argue more generally that selective non-response is not a concern for our estimations using the discount factor as the outcome by showing that the frequency with which an individual answers the discount factor question is not correlated with education and patience itself. If more patient individuals, particularly in the control group, are more likely to continue responding to the patience elicitation question in each survey year, then this could create a misleading negative correlation between being shaken by the earthquake and patience. To address this concern, Panel a of Figure 11 plots the average discount factors against the number of years an individual answers the discount factor question (ranging from 1 to 4 years). The figure indicates that individuals who respond more frequently do not differ significantly in patience from those who respond less often. Panel c further shows that in the shaken group, individuals with 4 years of responses are not statistically different from those with 1 or 2 years of responses.

Another check we do is to correlate the discount factor and the years of response to the patience elicitation question. In our estimation sample, household heads in the shaken group are more educated than those in the control group (19% of heads in the shaken group had a university degree, while only 11% of unshaken heads did). Since more cognitively able individuals tend to be more patient (Dohmen et al., 2010), if individuals with higher cognitive ability were more likely to keep answering the patience elicitation question, then this could bias the correlation between being shaken by the earthquake and patience upwards, creating a misleading positive correlation between being shaken and patience (the more educated heads in the shaken group are more likely to answer, pulling up their patience relative to the control group). Panel b of Figure 11 shows that individuals who had more years of responses to the patience elicitation question are not statistically different from those who had fewer responses in terms of years of education. Panel d shows that within the shaken group, individuals who had more years of responses to the discount factor question are not, on average, more educated than those with fewer responses.

APPENDIX C: PREFERENCE MEASURES

C.1. *Time preference with risk neutrality*

Assume that the expected utility theory (EUT) holds, and discounting is exponential; then the respondent is indifferent between receiving two income options M_{t+1}, the household's yearly revenue, and $M_t = (1-c)M_{t+1}$, where c is the cutoff value of the respondent, the percentage that she is willing to give up in order to collect the money immediately. She is indifferent between the two income options, receiving M_t today and nothing in a year or receiving nothing today and M_{t+1} in a year:

$$U(b+M_t)+\frac{1}{1+r}U(b)=U(b)+\frac{1}{1+r}U(b+M_{t+1}), \tag{17}$$

where b is background consumption, r is the discount rate, 1 unit of time is one year, and the utility function U is separable and stationary over time. Thus, equation (17) equalizes the present value of the utility of two monetary payments M_t and M_{t+1}. $\beta=\frac{1}{1+r}$ is time preference. Note that even though the monetary payments are unconditional and certain, the future background consumption is unavoidably uncertain.

We do not have a quantitative measure of risk aversion; therefore, we derive a simple time preference measure assuming risk neutrality. In this case equation (17) simplifies to

$$M_t=\frac{1}{1+r}M_{t+1}. \tag{18}$$

Then, using the cutoff value of the respondent, we can calculate her discount rate, using $M_t = (1-c)M_{t+1}$ as

$$1-c=\frac{1}{1+r} \tag{19}$$

C.2. *Time preference without risk neutrality*

In this section, we explain how we use risk aversion measures to correct the time preference measure for the curvature of the utility function. Table XI reports the results. First, we use an Arrow-Pratt risk aversion measure elicited in 2010 to estimate a relationship between this measure and the investment attitude question that were elicited since 2004. Next, we impute Arrow-Pratt risk aversion measures for other periods as well using our quantitative measure. We control for other household observables and regional characteristics as well. Finall,y we use the imputed Arrow-Pratt risk aversion measure to correct our baseline time preference measure.

In 2010, SHIW elicited risk aversion with the following question.
"Imagine you can take part in a lottery in which for every euro invested, you can either double your money (win 1 euro) or lose half of it (lose 50 cents), depending on a coin toss (tails you win, heads you lose). How much money would you invest?"

To calculate the Arrow—Pratt measure of relative risk aversion. Respondents are indifferent between not buying the lottery and buying it for a price z, i.e.

$$u(c)=(1-\pi)u(c-0.5z)+\pi u(c+z) \tag{20}$$

A second-order Taylor approximation around $u(c)$ gives

$$u(c) = (1-\pi)[u(c) + u'(c)(-0,5z) + \frac{1}{2}u''(c)(-0,5z)^2] + \pi[u(c) + u'(c)(z) + \frac{1}{2}u''(c)z^2] \tag{21}$$

$$0 = (1-\pi)\left[(-0,5z) + \frac{1}{2}\frac{u''(c)}{u'(c)}(-0,5z)^2\right] + \pi\left[z + \frac{1}{2}\frac{u''(c)}{u'(c)}z^2\right] \tag{22}$$

Assuming a CRRA utility, with γ Arrow—Pratt measure of relative risk aversion.

$$u(c) = \frac{c^{1-\gamma}}{1-\gamma}. \tag{23}$$

(21) simplifies to

$$0 = (1-\pi)\left[(-0,5z) + \frac{-\gamma}{2c}(-0,5z)^2\right] + \pi\left[z + \frac{-\gamma}{2c}z^2\right]. \tag{24}$$

Rearranging this leads to

$$\gamma = \frac{4c(3\pi - 1)}{z(1+3\pi)} \tag{25}$$

Next, we use the 2010 observations to establish a relationship between the Arrow-Pratt risk aversion measure and the investment attitude question. We impute Arrow-Pratt risk aversion measures $\hat{\gamma}_{it}$ for the remaining years, using the estimated relationship in 2010. As a result, each respondent will have an imputed absolute risk aversion measure for each time period, when she answered to the investment attitude question.

We use the imputed Arrow-Pratt risk aversion measure $\hat{\gamma}$ in (17) to generalize the discount factor measure. For background consumption b we use contemporaneous nondurable consumption

$$(b+M_t)^{1-\hat{\gamma}} + \frac{1}{1+r}(b)^{1-\hat{\gamma}} = (b)^{1-\hat{\gamma}} + \frac{1}{1+r}(b+M_{t+1})^{1-\hat{\gamma}}. \tag{26}$$

Rearranging gives

$$\beta = \frac{1}{1+r} = \frac{(b)^{1-\hat{\gamma}} - (b+M_t)^{1-\hat{\gamma}}}{(b)^{1-\hat{\gamma}} - (b+M_{t+1})^{1-\hat{\gamma}}}. \tag{27}$$

For $\hat{\gamma} = 1$ this simplifies to (19).

APPENDIX D: MECHANISMS ADDITIONAL RESULTS

Figure 17 presents heterogeneous effects of the earthquake on stated time preferences.

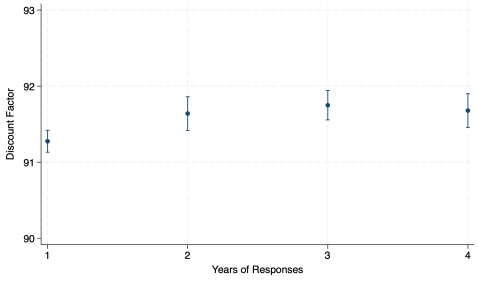
We first examine heterogeneity by pre-earthquake economic circumstances. The earthquake induced larger increases in impatience among individuals with lower pre-shock income or

wealth. Similar effects are observed for non-homeowners, consistent with their lower average wealth. These patterns align with our proposed mechanism: worse financial conditions increase vulnerability to stress (Carvalho et al. (2016), Haushofer and Fehr (2014)), which in turn heightens present focus.

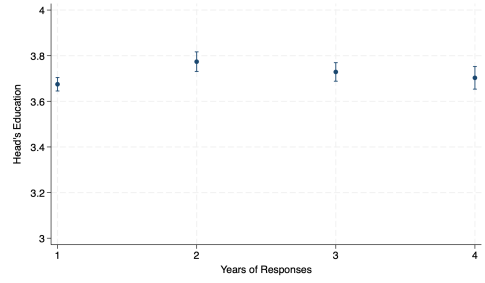
Next, we explore heterogeneity by cognitive capacity. We use financial literacy, measured in 2006 via two of the "Big Three" questions (Lusardi and Mitchell (2011)) testing understanding of inflation and interest compounding. Respondents who answered both correctly are classified as financially literate. Figure 17 shows that the decline in patience is concentrated among those who answered incorrectly. Similar patterns are found with education: individuals without a university degree experience significantly larger effects. These findings are consistent with theories of cognition under stress, where cognitive costs, shaped by education and literacy, increase vulnerability to shocks (Becker and Mulligan (1997); Gabaix and Laibson (2021); shown empirically by Dohmen et al. (2010)).

Finally, we find that individuals with lower pre-earthquake patience exhibit the largest declines post-shock. Since baseline patience is strongly correlated with income, wealth, education, and financial literacy, it may serve as a summary measure of underlying vulnerability. Taken together, these results suggest that the largest effects are concentrated among groups already most susceptible to stress. The patterns reveal a close interplay between stress, cognitive, and economic burdens.

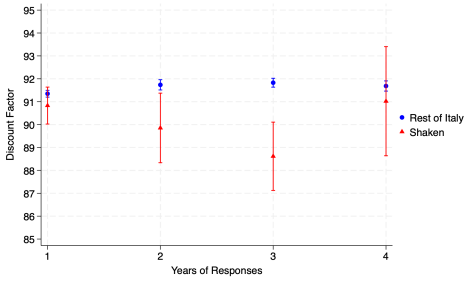
APPENDIX E: FIGURES AND TABLES



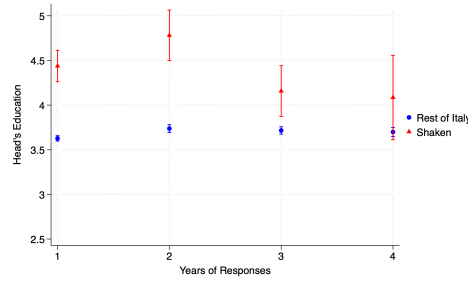
(a) Years of Responses and Education



(b) Years of Responses and Discount Factor



(c) Panel a by Treatment



(d) Panel b by Treatment

FIGURE 11.—Selective Non-Response.*Notes*: The figures are binned scatter plots of the time preference measure and number of years of responses. Panel A plots the average measured discount factor against the number of years an individual responds to the time preference survey question in the SHIW. Panel B plots the average years of completed education against how many years an individual responds to the time preference survey question. Panel C repeats in the analysis in Panel A by the treatment and control groups. Panel D repeats the analysis in Panel B by the treatment and control groups. All bars indicate 95% confidence intervals.

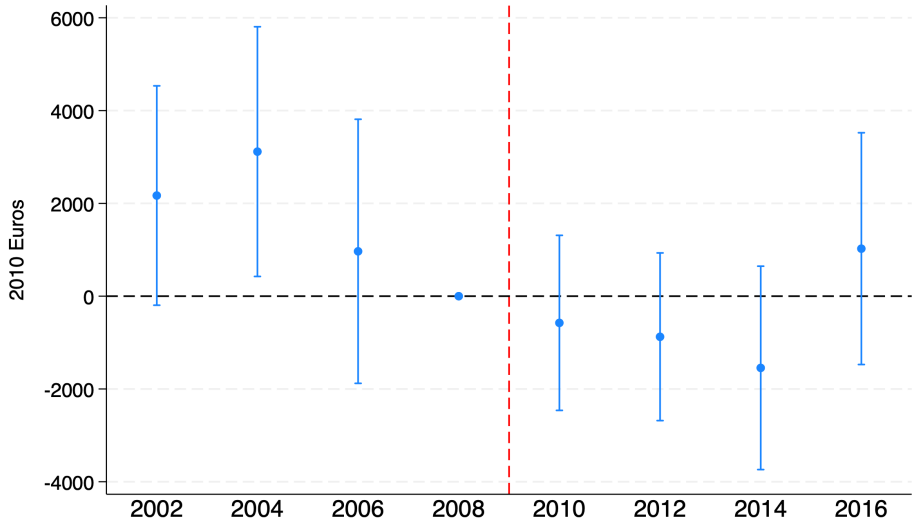


FIGURE 12.—Estimated Income Changes - Spatial Control. *Notes*: The figure plots the event study estimates of real income (2010 prices), when we restrict the analysis sample to individuals living in municipalities that are within 125 km from the epicenter. The reference period is the SHIW 2008 wave.

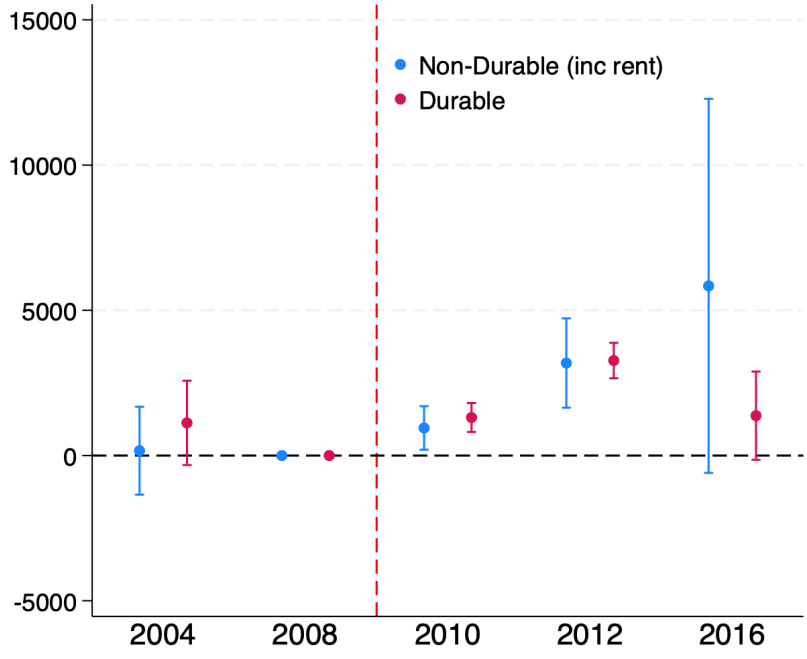


FIGURE 13.—Effects on Consumption (Unconstrained Households).

Note: The figure plots the event study estimates of the effects of being shaken by the earthquake on annual durable and non-durable consumption. Non-durable consumption includes actual rent but excludes imputed rent. We restrict the sample to include households who were non-HTM in all pre periods with data available prior to the earthquake. Household heads must also be between 25 to 70 years old at the time of the survey. Due to data limitations in the earlier and later waves of the SHIW, we pool the 2002 and 2004 waves, 2006 and 2008, and 2014 and 2016 for estimation.

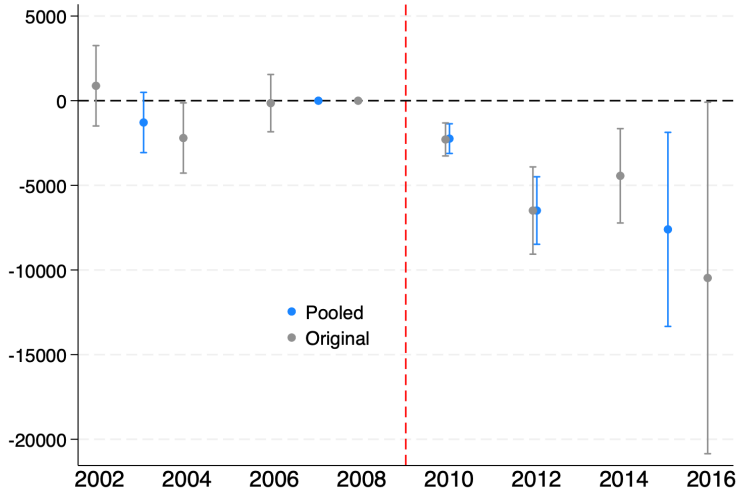


FIGURE 14.—Pooled vs Original Event Study - Savings).

Note: The figure plots the event study estimates of the effects of being shaken by the earthquake on annual savings. We restrict the sample to include households who were non-HTM in all pre periods with data available prior to the earthquake. Household heads must also be between 25 to 70 years old at the time of the survey. Due to data limitations in the earlier and later waves of the SHIW, we pool the 2002 and 2004 waves, 2006 and 2008, and 2014 and 2016 for estimation (blue). Our results remain robust when using the original individual years (grey) which are slightly less precise.

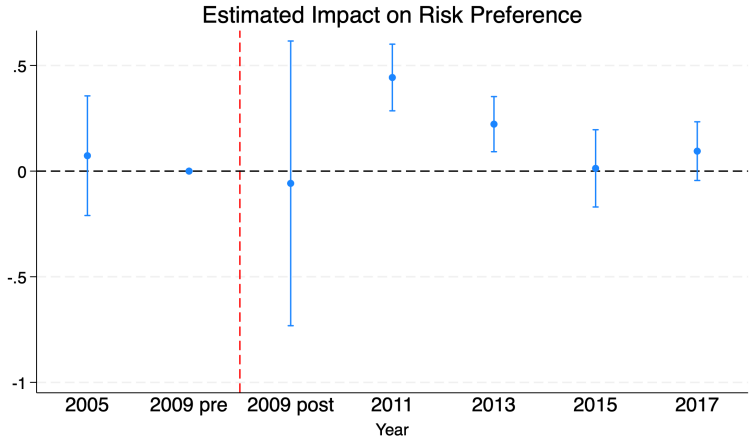
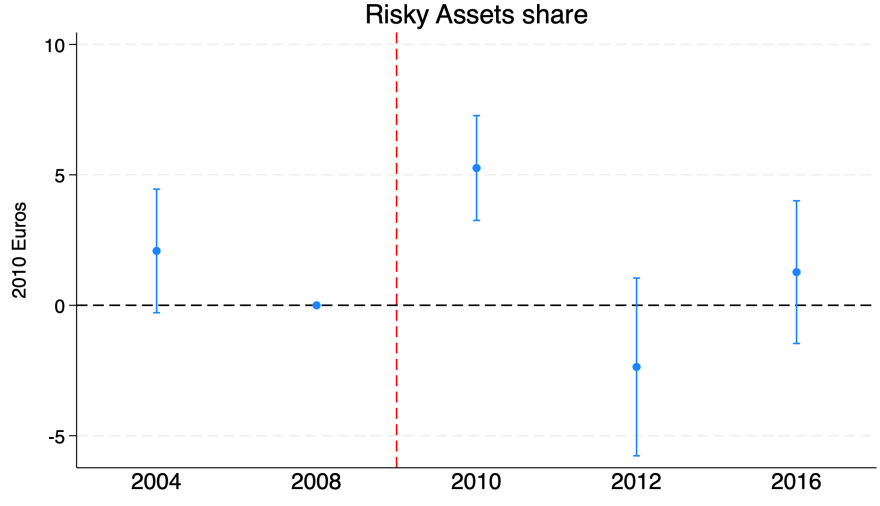


FIGURE 15.—Risk Preference. *Notes*: The figure plots the event study estimates of results of estimating equation 14 using the TWFE estimator on risk aversion. The measure is a categorical variable between 1 and 4, with 1 being the most risk-loving and 4 being the most risk averse. The y-axis shows the estimated effects of the 2009 L'Aquila earthquake, where positive values represent higher risk aversion. In this framework, the reference periods are the 2009 interview dates before 6 April, 2009 in the SHIW 2008 wave.



(a) Safe Assets



(b) Risky Assets

FIGURE 16.—Effect on Safe and Risky Asset Shares - Event Study

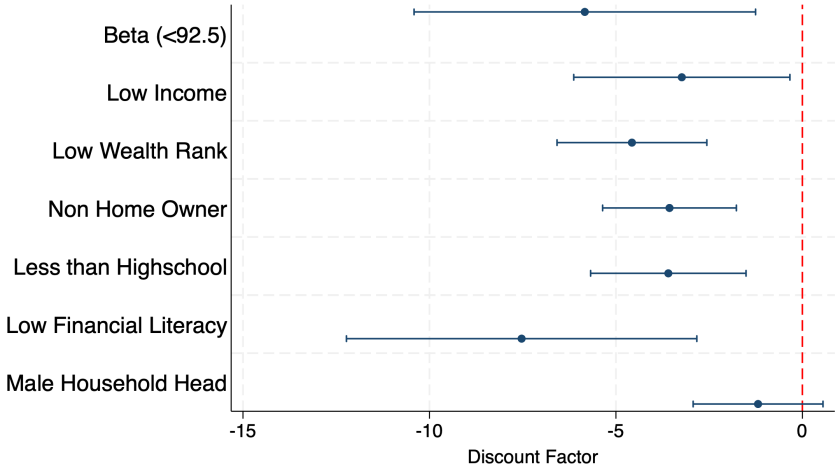


FIGURE 17.—Heterogeneous Impact of the Earthquake. *Notes*: The figure plots the coefficients of the interaction terms between the treatment indicator *Shaken* × *After* and an indicator variable for the respective characteristic of interest from estimating the difference-in-differences specification 13.

TABLE VI

PRE-EARTHQUAKE DESCRIPTIVE STATISTICS OF KEY VARIABLES

	Shaken (1)	*p-value* (2)	<40k (3)
Head characteristics			
Discount factor (0-100)	91.80	0.021	88.56
Male	0.65	0.887	0.64
University degree (dummy)	0.18	0.299	0.13
Employed (dummy)	0.65	0.358	0.60
Household characteristics			
Annual household income	27,853	0.282	30,131
Annual savings to income ratio (0-1)	0.21	0.618	0.24
Wealth	256,746	0.065	331,623
Family size >2 (dummy)	0.37	0.471	0.42
Have children (dummy)	0.20	0.363	0.15
No. of households	511	-	71

Notes: The table reports the pre-earthquake average outcomes of individuals living in shaken (excluding L'Aquila) municipalities (column 1) and those within 40 km (including L'Aquila) from the epicenter (column 3). The p-value column (2) shows the p-values of the hypotheses that the mean outcome of the group in the subsequent column is equal to the mean outcome of individuals in the shaken municipalities. The pre-earthquake data on the discount factor are from the 2004 and 2008 waves of the SHIW (in 2008, only responses from individuals interviewed before the 2009 Earthquake are used). Pre-earthquake data on other variables come from the 2002, 2004, 2006 and 2008 waves. Monetary values are in 2010 euros. Survey weights are used.

TABLE VIIDescriptive Statistics of Pre-Earthquake Characteristics: Stayed vs Attrited Households

	Shaken			Rest of Italy			Unshaken (<125 km)		
	Stayed	Attrited	*p-value*	Stayed	Attrited	*p-value*	Stayed	Attrited	*p-value*
Number of households	84	156		3791	5871		160	425	
Head characteristics									
Discount factor (0-100)	90.79	92.08	0.1437	92.02	91.94	0.5648	91.91	91.45	0.4623
Age at 2009 Earthquake	61.99	60.40	0.4191	59.77	59.67	0.7247	60.90	59.06	0.1532
Male	0.68	0.64	0.5222	0.72	0.70	0.0186	0.74	0.74	0.9077
University degree (dummy)	0.19	0.19	0.8805	0.11	0.10	0.2392	0.11	0.08	0.2696
Employed (dummy)	0.50	0.58	0.2402	0.53	0.55	0.0400	0.50	0.60	0.0078
Household characteristics									
Household income (2010 euros)	28,733	29,569	0.7230	28,923	27,622	0.0072	27,578	27,909	0.8389
Davings to income ratio (0-1)	0.24	0.25	0.7326	0.24	0.21	<0.0001	0.24	0.20	0.1277
Family size >2	0.42	0.35	0.2184	0.48	0.44	<0.0001	0.51	0.54	0.4200
Number of working family members	1.70	1.54	0.0850	1.72	1.65	<0.0001	1.79	1.81	0.7942
Have children	0.15	0.13	0.6737	0.21	0.19	0.0094	0.22	0.19	0.4167

TABLE VIII

EFFECT ON PATIENCE - ROBUSTNESS

	Rest of Italy				Spatial Control		
	All	No Rome	No ER	Interval	All	No Rome	Interval
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Shaken x After	-6.232	-5.338	-5.540	-1.374	-7.652	-7.305	-1.668
	(0.790)	(3.215)	(0.711)	(0.393)	(1.398)	(3.927)	(0.548)
Respondent FE	yes	no	no	no	yes	no	no
Household FE	no	yes	yes	yes	no	yes	yes
Mean of dep. var.	92	92.1	92	91.7	91.7	92.6	92
Observations	10,075	11,897	10,914	22,378	1,059	1,174	2,831
R-squared	0.555	0.519	0.525	-	0.610	0.480	-

Note: The dependent variable is the discount factor (0-100), our time preference measure. The table reports the difference-in-differences estimates of the impact of being shaken by the 2009 Earthquake in columns 1-3 and 4-6. Columns 4 and 7 report interval estimations, where we use the upper and lower bounds of the elicited time preference answers as the dependent variable. The 2004, 2008, 2010 and 2012 waves of the SHIW are used as time preference is elicited in these years. The analysis sample excludes households whose heads were younger than 25 or older than 90 at the time of the Earthquake. Household characteristics controlled for include age and education of the household head, family size and whether the household has children. All specifications include initial municipality and year fixed effects. Clustered standard errors at the initial municipality of residence are in parentheses.

TABLE IX

SHORT-RUN EFFECTS ON MONTHLY DURABLES CONSUMPTION - NON HAND-TO-MOUTH HOUSEHOLDS

	OLS		DiD				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Shaken X After			106.6	1312.6	111.6	109.2	107.8
			(15.02)	(181.5)	(15.30)	(15.13)	(14.83)
Shaken	54.29	50.63					
	(14.14)	(14.82)					
Mean of dep. var.	141	142	187	2254	188	188	188
Observations	4,795	4,760	16,523	16,349	16,349	16,349	16,349
R-squared	0.047	0.050	0.368	0.369	0.369	0.369	0.369
Income	yes	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	yes	yes	yes
Liquid asset	no	no	no	no	yes	no	no
Wealth	no	no	no	no	no	yes	yes
Credit constraint	no	no	no	no	no	no	yes

Note: The dependent variable is monthly consumption of durable goods. All values are in 2010 euros. Columns 1 and 2 report OLS estimates using post-earthquake data only. Columns 3 to 7 report difference-in-differences (DD) estimates of the impact of being shaken by the 2009 Earthquake. The 2002, 2004, 2006, 2008, 2010, and 2012 waves of the SHIW are used to examine the impact of monthly spending in the short run (up to 3 years after the earthquake). The analysis sample excludes households whose heads were younger than 25 or older than 90 at the time of the Earthquake. We further restrict the sample here to those aged between 25 to 70 at the time of the survey, and to those who were non-HTM prior to the earthquake. Household characteristics controlled for include age and education of the household head, family size and whether the household has children. OLS models include year fixed effects; DiD models include household, initial municipality and year fixed effects. Clustered standard errors at the initial municipality of residence are in parentheses.

TABLE X

LONG RUN PORTFOLIO ALLOCATIONS

Ranking	Liquid Asset	Financial Asset	Bank Liabilities
	(1)	(2)	(3)
Δ discount factor (post)	0.712	0.820	-0.633
	(0.198)	(0.177)	(0.299)
discount factor (pre)	0.896	1.062	-0.600
	(0.285)	(0.230)	(0.435)
Year FE	yes	yes	yes
Municipality FE	yes	yes	yes
Mean of dep. var.	52.9	53.3	22.8
Observations	1,553	1,553	1,553
R-squared	0.438	0.545	0.317

Note: The table reports estimated coefficients of post-earthquake change in the stated discount factor on portfolio allocations. The dependent variables are rankings of liquid assets (column 1), financial assets (column 2) and bank liabilities (column 3). The 2014 and 2016 SHIW waves are used to determine effects on long term portfolio choices. The analysis sample excludes households whose heads were younger than 25 or older than 70 at the time of survey. Household characteristics controlled for include age and education of the household head, family size and whether the household has children. Clustered standard errors at the initial municipality of residence are in parentheses.

TABLE XI

EFFECTS ON PATIENCE (RISK AVERSION CORRECTED) - DIFFERENCE-IN-DIFFERENCES

	Discount Factor	Corrected Discount Factor	Imputed Risk
	(1)	(2)	(3)
Shaken X After	-5.64	-3.933	-0.0155
	(0.724)	(0.521)	(0.0144)
Real lifecycle income ('000)	0.0330	0.0436	0.00226
	(0.0148)	(0.0108)	(0.000413)
Mean of dep. var.	92.2	94.5	1.04
Observations	11,816	11,816	14,450
R-squared	0.564	0.572	0.918

Note: The table reports difference-in-differences estimates on the effect of being shaken on various measures of the discount factor (columns 1 and 2) and on imputed risk (column 3). The analysis sample excludes households whose heads were younger than 25 or older than 90 at the time of the Earthquake. Household characteristics controlled for include age and education of the household head, family size and whether the household has children. All specifications include household, initial municipality and year fixed effects. Clustered standard errors at the initial municipality of residence are in parentheses.

TABLE XII

HETEROGENEITY BY AGE, EDUCATION AND SUBJECTIVE HEALTH

	Stated Discount Factor		
	(1)	(2)	(3)
Shaken x After	-9.512	-7.782	-4.787
	(1.402)	(0.531)	(0.562)
Shaken x After x Age (25 to 45)	7.809		
	(1.131)		
Shaken x After x Age (46 to 70)	2.814		
	(1.991)		
Shaken x After x University		6.594	
		(0.784)	
Shaken x After x Very Poor health			-11.399
			(0.583)
Shaken x After x Poor or Fair health			-4.505
			(1.612)
Mean of dep. var.	92.1	92.1	92
Observations	13,688	10,911	10,925
R-squared	0.523	0.497	0.480

Note: The table reports difference-in-differences estimates on the effect of being shaken on the discount factor. The Shaken x After is interacted with age bins (column 1), a binary variable indicating university education (column 2), and categorical variables indicating very poor, poor, fair, good or excellent subjective health pre-earthquake (column 3). The reference (omitted) age bin is 71 to 90 years old. The analysis sample excludes households whose heads were younger than 25 or older than 90 at the time of the Earthquake. Household characteristics controlled for include age and education of the household head, family size and whether the household has children. All specifications include household, initial municipality and year fixed effects. Clustered standard errors at the initial municipality of residence are in parentheses.

TABLE XIII

SUBJECTIVE HEALTH, AGING AND THE EARTHQUAKE

	Self-Reported Health (1)
Shaken × After	-0.242
	(0.0311)
Age	-0.0248
	(0.000709)
Controls	yes
Income	yes
Mean of dep. var.	3.97
Observations	21,186
R-squared	0.380

Note: The table reports reduced-form estimates on self-reported health, which are elicited in the 2006, 2008 and 2010 waves. Subjective health is reported on a scale from 0 to 5, with 0 indicating very poor health and 5 indicating excellent health. Household characteristics controlled for include age and education of the household head, family size and whether the household has children. The specification includes household, initial municipality and year fixed effects. Clustered standard errors at the initial municipality of residence are in parentheses.

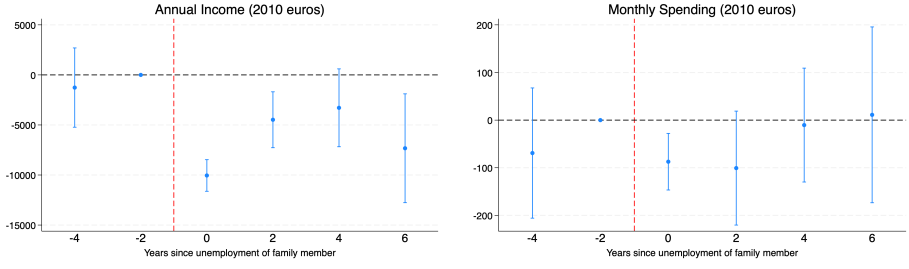
APPENDIX F: MPC ESTIMATION FROM UNEMPLOYMENT SHOCKS

To benchmark the consumption response to the earthquake against a standard income shock, we estimate the marginal propensity to consume (MPC) out of labor income using pre-earthquake SHIW data (2002–2008), following the identification strategy of Patterson (2023). We estimate:

$$\Delta C_{it} = \alpha + \alpha^{mpc} \Delta Y_{it} + \delta_t + \mathbf{X}'_{it}\gamma + \epsilon_{it}, \tag{28}$$

where ΔC_{it} is the two-year change in non-durable consumption and ΔY_{it} is the two-year change in disposable income for household i between SHIW waves. We instrument ΔY_{it} with an indicator for job loss by any household member between waves, restricting the sample to households employed in the prior wave. Year fixed effects δ_t absorb aggregate time variation. Controls $\mathbf{X}_{it}$ include household head age, education, family size, and lagged income. Standard errors are clustered at the municipality level.

The IV estimate of α^{mpc} is 0.37, with a first-stage F-statistic of [XX], confirming instrument relevance. In the first year of unemployment, the average household loses approximately €830 per month (Appendix Figure 18), implying a consumption decline of roughly €300 per month. This estimate is somewhat below the 0.5 reported by Patterson (2023) for the United States using the PSID, consistent with Italy's stronger automatic stabilizers compressing the net income loss from job separation. The sample is restricted to non-HTM households aged 25–70, matching the consumption analysis in Section 5.2.



(a) Income (b) Monthly Spending

FIGURE 18.—Effects of Unemployment - Event Study