

COVID-19 lockdown duration and transitions in family intergenerational support patterns: Evidence from the China Family Panel Studies

Jun He

jhe_20000@126.com

Shanghai Lixin University of Accounting and Finance

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Author:

Jun He(First Author & Corresponding Author)

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Affiliation

School of Public Finance & Administration, Shanghai lixin University of Accounting and Finance, Shanghai, China

E-mail

jhe_2000@126.com

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Abstract: The family is the primary informal insurance mechanism through which economies buffer individual and aggregate risks, yet whether a major crisis activates intergenerational mutual aid or, instead, weakens intergenerational ties through physical isolation and resource strain remains unclear. Existing studies mostly examine changes in a single support dimension in isolation, or the simple before-versus-after correlation, and often rely on small, short-run samples, leaving the overall effect of the shock on the configuration of intergenerational support poorly understood. Using four waves (2016–2022) of the China Family Panel Studies (CFPS), we first classify intergenerational support into four cross-wave comparable patterns—Reciprocal, Filial-support, Interactive, and Detached—through latent class analysis, and then apply a multi-group difference-in-differences design to identify the causal effect of lockdown intensity on transitions between support patterns, its dynamic path, and its heterogeneity by household resources. We find that prolonged lockdowns shift households away from one-way support or detachment toward two-way reciprocity: the effect increases in lockdown intensity, is concentrated in the most strictly locked-down regions, and raises the post-pandemic probability of being Reciprocal by about 4.8 and 5.1 percentage points on average. The effect is delayed, being close to zero in the shock year (2020) and significant only by 2022. The shift toward reciprocity is widespread across income groups, whereas transitions into Filial-support are highly concentrated among low-income households. Overall, a major public-health shock triggers an adaptive reconfiguration of intergenerational support patterns, but its direction, lag, and form depend on shock intensity and household resources; the family, as an informal safety net, is resource-asymmetric, all the more so for vulnerable households.

Keywords: COVID-19 lockdown intensity; intergenerational support patterns; income heterogeneity

JEL Classification: J12; D19; I18; J14

1 Introduction

The family is a central informal risk-sharing mechanism in most economies. When labor markets are disrupted and public services contract, intergenerational financial transfers, time-based

care, and emotional ties constitute the primary resource on which individuals rely to smooth consumption and welfare (Townsend, 1994; Rosenzweig & Wolpin, 1993; Attanasio & Pistaferri, 2016; Fafchamps, 2011; Frankenberg, Smith, & Thomas, 2003). Yet when a single shock simultaneously erodes household members' income, time, and ability to meet face to face, whether this safety net is activated by the crisis or contracts under resource strain remains an open question, and its eventual form need not amount to a simple change in the total volume of support. In the rapidly aging, increasingly small-family East Asian societies with a strong familist tradition, intergenerational support has long been the backbone of old-age support and living arrangements (Logan & Bian, 1999; Cai, Park, & Yip, 2022); identifying how a major exogenous shock reshapes this backbone is therefore of substantial theoretical and policy relevance.

Why intergenerational support exists and how it evolves with the environment depends on its underlying motives and structure. Altruistic models hold that transfers rise with the recipient's needs, enabling family members to maximize joint utility and share risk (Becker, 1974; Altonji, Hayashi, & Kotlikoff, 1992). Exchange and strategic-motive models instead stress that transfers are conditioned on reciprocal returns or bequest incentives, and that their sustainability depends on the resources of both parties (Cox, 1987; Bernheim, Shleifer, & Summers, 1985; Cox & Fafchamps, 2007). Intergenerational solidarity theory further argues that intergenerational relations are jointly constituted by economic, functional, and emotional dimensions and appear over the life course as a dynamic combination of reciprocity and support (Bengtson & Roberts, 1991; Silverstein et al., 2002). Placed under an exogenous shock, these theories yield non-unique predictions: altruism and risk-sharing imply that the crisis strengthens intergenerational solidarity, whereas exchange and family-stress mechanisms predict that resource losses and spatial isolation contract support. Which motive prevails—and into what configuration multidimensional support is ultimately reorganized—is theoretically open and must be identified through exogenous variation in intensity and multidimensional measurement (see Section 2).

Research on the demographic and economic consequences of COVID-19 offers preliminary evidence on fertility fluctuations (Kearney & Levine, 2023), time allocation and remote work (Cowan, 2024), the health costs of intergenerational care (Eibich & Zai, 2024), and the welfare of left-behind parents in China (Cai, Park, & Yip, 2022); the labor-market consequences of the shock have been highly unequal across groups with different resources (Adams-Prassl et al., 2020), and a life-course perspective likewise emphasizes that major shocks reshape family relations through linked lives (Settersten et al., 2020). Among these studies, those that directly examine intergenerational support within families during the pandemic are closest to ours and most clearly reveal the gaps in the existing evidence.

The studies most directly related to ours examine adult children's support to their parents during the pandemic. Using U.S. Health and Retirement Study (HRS) data from 2016 – 2020, Lin et al. (2025) find that adult children became more responsive to specific parental needs during the pandemic: parents facing financial difficulty were more likely to receive monetary help, and those facing non-financial procurement difficulty more likely to receive time help. Drawing on an online survey conducted in the early stage of the pandemic in Germany, Klingel et al. (2025) find that about one-third of adult children reduced or interrupted their upward support to parents. These studies provide an important starting point but share three limitations. First, they examine monetary and time support dimension by dimension and do not treat two-way, multidimensional support as a jointly configured “support bundle” (Ho & McGarry, 2025), so they cannot identify type

transitions at the pattern level. Second, their identification relies on before-versus-after associations or early-pandemic cross-sections rather than exogenous regional variation in lockdown intensity, making it difficult to rule out confounders such as common age trends and precluding a dose – response relationship. Third, their observation windows mostly end in the first year of the pandemic, leaving the medium-run evolution of pattern reconfiguration—and its resource stratification across households of different economic status—largely undocumented.

This study address these gaps using four waves (2016–2022) of the CFPS. We first use LCA to summarize intergenerational support across the economic, instrumental, and emotional dimensions into four cross-wave comparable patterns: Reciprocal, Filial-support, Interactive, and Detached. We then measure lockdown intensity by the natural duration of each province's Level-I public health emergency response and, exploiting this provincial intensity variation—which is exogenous to households' pre-existing characteristics—identify the causal effect of lockdown on household pattern transitions through a multi-group DID, characterize the dynamic path with an event study, and identify resource heterogeneity through interactions with pre-pandemic baseline household income.

We obtain three findings. First, prolonged lockdowns shift households from one-way support or detachment toward two-way reciprocity, and the effect depends strictly on shock intensity: relative to short-lockdown regions, long-lockdown regions raise the post-pandemic probability of being Reciprocal by about 4.8 and 5.1 percentage points on average, the effect in medium-lockdown regions is small, and continuous lockdown days exhibit a monotonic dose – response relationship. Second, the effect is delayed: it is close to zero in the shock year (2020) and emerges only in the second post period (2022), while pre-treatment coefficients support parallel trends. Third, the reconfiguration exhibits a clear resource gradient: the concentration toward two-way reciprocity occurs across income groups, whereas responding to the crisis through a one-way Filial-support pattern is concentrated in low-income households.

This study makes three contributions. First, it extends the literature on intergenerational transfers and family risk sharing (Becker, 1974; Cox, 1987; Townsend, 1994) by moving the unit of analysis from the quantity of a single-dimension transfer to the configuration of a multidimensional support bundle, showing that the effect of a shock is not a simple change in support volume but a reorganization of how support is arranged under resource constraints, and providing typological evidence on the coexistence of altruistic and exchange motives. Second, to the literature on the demographic and economic consequences of the pandemic (Kearney & Levine, 2023; Eibich & Zai, 2024; Cowan, 2024), we exploit an exogenous, continuous, time-invariant treatment—provincial lockdown intensity—to deliver causal evidence that combines dose – response, dynamic path, and resource heterogeneity. Third, we show that a crisis can broadly activate two-way reciprocity, while the most vulnerable households can only respond with one-way upward filial support, which informs the complementary design of formal social protection and family support.

2 Theoretical frameworks

2.1 The effect of pandemic shock on transitions between intergenerational support patterns

An intergenerational support pattern is a relatively stable equilibrium of coordinated multidimensional support that a family forms under given resources and institutional conditions (Bengtson & Roberts, 1991; Silverstein et al., 2002; Ho & McGarry, 2025). In normal times, financial transfers, daily care, and emotional contact repeat along stable household divisions of labor and behavioral expectations, allowing a family to persist in a given type. Such persistence, however,

presumes that the external environment and household resources remain broadly unchanged. A large-scale, exogenous aggregate shock that simultaneously hits income, time, and spatial interaction alters both the demand structure and the supply constraints of support and thereby breaks the prevailing equilibrium (Elder, 1999; Settersten et al., 2020). Family stress theory holds that external stressors force families to reorganize internal resources and roles (Conger & Elder, 1994; Conger & Conger, 2002), and the risk-sharing perspective likewise emphasizes that adverse shocks alter the direction and scale of intra-family transfers (Townsend, 1994; Dercon, 2002). When several dimensions of support must adjust simultaneously, a family is more likely to move between types than merely to vary support quantities within its original type.

On this basis, we formulate a baseline hypothesis. H1: The COVID-19 shock may cause household intergenerational support patterns to transition between types.

There are, however, two possible paths of transition. Along one path, the crisis activates the family's informal-insurance and reciprocity norms, strengthening multidimensional support in both directions and converging toward the Reciprocal type. When labor earnings fluctuate while paid and public services contract simultaneously, families can no longer rely on external provision and turn inward for insurance (Townsend, 1994; Fafchamps, 2011). Driven by altruism and reciprocity, as one party's needs rise, the other's transfers and care increase, in expectation of reciprocal support when its own needs arise (Becker, 1974; Arrondel & Masson, 2006; Laferrère & Wolff, 2006). Disaster research shows that after an adverse shock families often build mutual-aid arrangements through co-residence, expanded intergenerational transfers, and joint coping (Frankenberg, Smith, & Thomas, 2003; Sawada & Shimizutani, 2008); the linked-lives perspective likewise anticipates that a crisis strengthens bonds and mutual responsibility among family members (Elder, 1999; Settersten et al., 2020). Along this path, parents and adult children provide mutual support across the economic, instrumental, and emotional dimensions, upward and downward transfers strengthen at the same time, and the family migrates from a one-way or loose configuration to a two-way reciprocal one; during the pandemic, adult children have likewise been observed to become more responsive to specific parental needs (Lin et al., 2025).

An opposite path is equally plausible. The same shock may simultaneously weaken both generations' capacity to provide support, contracting multidimensional support in tandem and moving the family toward detachment. On the economic dimension, pandemic-induced income losses, unemployment, and business difficulties compress the resources available for transfers; under exchange and strategic motives, transfers are conditioned on both parties' resources and expected returns, and when both are impaired and future returns become uncertain, the basis for transfers weakens (Cox, 1987; Bernheim, Shleifer, & Summers, 1985; Cox & Fafchamps, 2007). On the time and spatial dimensions, lockdowns, social distancing, and mobility restrictions directly disrupt face-to-face daily care and contact (Klingel et al., 2025), while working from home, school closures, and care burdens crowd out discretionary time, making instrumental support and emotional contact difficult to sustain. The family stress model further notes that persistent economic pressure and exhaustion breed relationship tension, emotional distancing, and support withdrawal (Conger & Elder, 1994; Conger & Conger, 2002); when formal care is cut off and intergenerational contact is restricted, existing support arrangements may be interrupted rather than substituted (Eibich & Zai, 2024). Along this path, upward and downward transfers, care, and contact all decline, and the family migrates from an integrated configuration to a Detached type marked by broadly missing support and loose intergenerational ties. Early-pandemic survey evidence likewise finds that about one-third

of adult children reduced or interrupted support to their parents (Klingel et al., 2025), offering an empirical clue in the opposite direction.

Following these two opposing theoretical paths, we build on H1 to formulate two competing hypotheses:

H1.1: The pandemic shock may push household intergenerational support patterns toward the Reciprocal type.

H1.2: The pandemic shock may push household intergenerational support patterns toward the Detached type.

2.2 The temporal effect of the pandemic shock on support-pattern transitions

Because an intergenerational support pattern is a stable equilibrium of coordinated multidimensional support, its reconstruction can hardly be completed at the instant of the shock; the effect may instead vary with distance from exposure and exhibit a dynamic temporal dimension. First, short-run buffers and adjustment costs induce delay. Facing a shock, households first draw on existing buffers — precautionary savings, borrowing, and temporary transfers — to maintain prevailing arrangements rather than immediately reorganizing support (Jappelli, 1990; Rosenzweig & Wolpin, 1993; Blundell, Pistaferri, & Preston, 2008; Attanasio & Pistaferri, 2016). A support pattern involves coordinated changes in living arrangements, the division of care, and income arrangements and therefore carries adjustment costs and behavioral inertia; the life-course perspective likewise notes that family trajectories typically change gradually rather than instantaneously (Elder, 1999; Settersten et al., 2020). Under incomplete information, when households cannot judge how long the shock will last, they tend to wait and adjust later, so pattern-level transitions may lag the shock itself. Second, the continuation of the shock and the revision of expectations may cause the effect to accumulate. Transitory income fluctuations can be absorbed by savings and informal borrowing; only when households recognize that the shock is becoming persistent and formal services remain absent do they upgrade temporary coping into a stable multidimensional reorganization, and the reconstruction of informal risk-sharing networks likewise tends to be completed only as the shock continues (Townsend, 1994; Dercon, 2002; Fafchamps, 2011). As family stress accumulates over time, relationship adjustments masked by buffers at the outset may surface in later stages (Conger & Conger, 2002), so the effect on pattern transitions may strengthen over time. Finally, economic recovery and family adaptation may cause the effect to decay. In contrast to delay and accumulation, as lockdowns are lifted and labor markets and formal services gradually recover, intergenerational support that was temporarily activated — or suppressed — by the shock may revert toward normal, and family resilience and adaptation may help households return to their original trajectories (Walsh, 2003); pattern transitions may then occur mainly in the shock period and weaken or even reverse afterward, appearing transitory rather than persistent.

Life-course theory stresses that the consequences of an event depend on its timing and duration, and that the same shock carries different meanings in the contemporaneous period than several years later (Elder, 1999; Settersten et al., 2020). Hence, whether the effect ultimately takes the form of delayed accumulation or emergence followed by decay, its magnitude and even its direction may vary over time (Goodman-Bacon, 2021; Rambachan & Roth, 2023). On this basis we formulate the second hypothesis:

H2: The effect of the pandemic shock on support-pattern transitions exhibits a temporal dimension.

2.3 Divergence by household economic conditions

Facing an exogenous shock of the same intensity, households with different economic resources may differ systematically in both the probability and the specific form of pattern transition. Households do not absorb the shock from the same starting point: economic resources accumulated before the pandemic alter the relative strength of the “support-activated” and “support-suppressed” mechanisms, and theory offers two opposing predictions about the direction of divergence.

First, economic resources underpin the family's informal insurance, and resource-rich households are better able to complete a multidimensional, two-way adaptive reorganization. Intergenerational financial transfers require disposable means, time care requires leisure and flexible schedules, and the simultaneous upward adjustment of several dimensions into two-way reciprocity demands buffers such as savings, assets, and stable income (Jappelli, 1990; Rosenzweig & Wolpin, 1993; Blundell, Pistaferri, & Preston, 2008; Attanasio & Pistaferri, 2016). Resource-rich households can also purchase market services to substitute for formal care disrupted by the pandemic (Bonsang, 2009), organizing members into a stable reciprocal arrangement. Resource-constrained households face the opposite situation: they confront a highly correlated common shock in which members of the risk-sharing network are typically hit simultaneously, leaving no party with spare capacity to transfer (Townsend, 1994; Dercon, 2002; Fafchamps, 2011; Morduch, 1994); liquidity constraints compress financial transfers and employment pressure crowds out care time, making it hard to sustain several dimensions at once, and family stress may further translate into relationship tension and support withdrawal (Conger & Elder, 1994; Conger & Conger, 2002; Ettner, 1995). Along this logic, disadvantaged households are less able to move toward two-way reciprocity and more likely to contract passively into one-way support or overall detachment.

Second, differences in the availability of formal protection and market substitutes yield an opposing prediction. When formal social security and paid services are insufficient, low-income households lack the means to purchase care, insurance, and services in the market, and the family network is almost the only risk-coping resource they can mobilize; they may therefore be forced to strengthen intergenerational co-residence and mutual aid, making the family the “last line of defense” (Logan & Bian, 1999; Whyte, 2003; Cai, Giles, O’Keefe, & Wang, 2012). High-income households can instead self-insure through savings, commercial insurance, and purchased services, substituting formal or market support for family intergenerational support (Bonsang, 2009; Charles & Sevak, 2005; Van Houtven & Norton, 2004), and their intergenerational support need not increase under the shock. Along this logic, economic conditions and family mutual aid may be inversely related: the fewer market substitutes a household has, the more it relies on and strengthens intergenerational ties.

What is clear, therefore, is that a major shock tends to amplify pre-existing resource inequalities across households, with consequences that are uneven across economic groups (Adams-Prassl et al., 2020; Elder, 1999; Settersten et al., 2020). We accordingly formulate the third hypothesis:

H3: The effect of the pandemic shock on support-pattern transitions differs across households with different economic conditions.

3 Data, Variables, and Research Strategy

3.1 Data

We use four waves (2016 – 2022) of the CFPS, chosen for two reasons. First, 2016 and 2018 constitute the pre-pandemic baseline; 2020 is the first shock period (fielded July – December of that year, roughly three to eight months after the lifting of the Wuhan lockdown); and 2022 is the

continuation period (fielded June – December, still under the dynamic zero-COVID framework). Together the four waves form a “pre – shock – continuation” quasi-experimental sequence that identifies both the contemporaneous effect and the medium-run evolution. Second, the six variables constituting the support pattern use consistent definitions across the four waves, so the identified patterns are comparable over time.

The unit of analysis is the household-year, and the sample is restricted to households with at least one parent alive, ensuring that an intergenerational support relationship exists. After cleaning missing values, the final panel contains 34,118 household-year observations for 10,067 unique households across 31 provincial-level regions.

3.2 Variables

The dependent variable is the intergenerational support pattern, built from three dimensions—economic, instrumental, and emotional support—and specifically six manifest variables: child-to-parent economic support, parent-to-child economic support, child-to-parent daily care, parent-to-child daily care, emotional closeness, and intergenerational contact. Data processing proceeds in two steps.

The first step is manifest-variable coding. The four functional support variables (two-way economic support and two-way daily care) were originally recorded as yes/no; we combine the answers regarding the father and mother for each and code it 1 if either is “yes” and 0 otherwise. Emotional closeness takes the higher of the relationship-rating scores with the father and the mother, coding 1 for a score of 4 or above and 0 otherwise. Intergenerational contact combines the two channels “meeting” and “contact by other means”: across the four frequency variables for both parents we take the most frequent, coding 1 for “at least weekly” and 0 otherwise.

The second step aggregates from individuals to the household: if any member scores 1 on a manifest variable, the household is coded 1; if all score 0 it is coded 0; and if all are missing it is set to missing.

After coding, we fit a latent class model on the six manifest variables separately for each year and obtain four mutually exclusive patterns. Because year-by-year LCA may exhibit label switching, we match classes across years by their conditional-probability profiles, ensuring that pattern labels refer to the same latent configuration across the four years.

Table 1 reports the year-by-year LCA fit statistics. The BIC is minimized at K=4 in all four years and the aBIC is likewise generally minimized at K=4, while the entropy of the four-class model is moderately high. A BLRT based on 300 parametric bootstraps shows that moving from three to four classes is significant at the 1% level in every year, whereas the likelihood-ratio gain from four to five classes decays sharply (the observed LR falls from 95.6–202.8 to 18.2–42.5), information criteria generally rise again at K=5, and a fifth class lacks a stable, replicable substantive meaning. Combining information criteria, entropy, BLRT, and substantive interpretability, the four-class solution is clearly and replicably identifiable across waves.

Table 1. Year-by-year latent class model fit statistics

Year	K	N	Log-lik	BIC	aBIC	Entropy	BLRT p
2016	2	10552	-33,479.2	67,078.9	67,037.6	0.535	<.01
	3	10552	-33,388.7	66,962.8	66,899.2	0.506	<.01
	4 ✓	10552	-33,287.3	66,824.8	66,739.0	0.696	<.01
	5	10552	-33,272.2	66,859.4	66,751.4	0.632	0.150
2018	2	9601	-30,331.9	60,783.1	60,741.7	0.539	<.01

	3	9601	-30,193.6	60,570.6	60,507.0	0.579	<.01
	4 ✓	9601	-30,100.1	60,447.7	60,361.9	0.578	0.003
	5	9601	-30,091.0	60,493.7	60,385.7	0.512	0.017
2020	2	7975	-25,362.0	50,840.9	50,799.6	0.544	<.01
	3	7975	-25,215.1	50,610.0	50,546.4	0.611	0.000
	4 ✓	7975	-25,167.3	50,577.2	50,491.4	0.696	<.01
	5	7975	-25,146.1	50,597.7	50,489.6	0.642	0.023
2022	2	7623	-24,361.1	48,838.5	48,797.1	0.566	<.01
	3	7623	-24,223.6	48,625.9	48,562.3	0.578	<.01
	4 ✓	7623	-24,142.7	48,526.8	48,441.0	0.597	0.007
	5	7623	-24,128.9	48,561.7	48,453.6	0.606	0.043

Notes: ✓ marks the model selected for each year; the BLRT p-value is a likelihood-ratio test based on 300 parametric bootstraps. We also fit a K=6 model whose BIC/aBIC rise further relative to K=4 and is therefore not reported.

3.3 Research strategy

We exploit differences across provinces in the natural duration of the Level-I emergency response to construct a multi-group DID design, with lockdown intensity as a multivalued treatment and the pre/post-pandemic division as the time dimension, to identify the causal effect of lockdown on household support patterns. The validity of this quasi-experimental strategy for identifying the causal effect of exogenous policy on household behavior has been established in prior work (Li & Shi, 2025), and multi-period DID under varying treatment intensity follows a well-developed estimation paradigm (Pilkaukas et al., 2024). The pandemic struck all households simultaneously in early 2020, whereas the duration of the Level-I response differed markedly across provinces; this duration was determined by exogenous exposure at the outset of the pandemic, could not be chosen by households, and cannot be reverse-influenced by household intergenerational behavior. The model includes household and year fixed effects, which absorb, respectively, time-invariant household heterogeneity and the common national shock. The specific specifications are as follows.

(1) Cross-sectional multi-group DID (main regression). For each class pair (k, l), we estimate on the corresponding two-class subsample:

$$y_i^{(k,l)}jt = \beta_L(Long_j \times Post_t) + \beta_M(Med_j \times Post_t) + \gamma'X_{ijt} + \alpha_i + \lambda_t + \varepsilon_{ijt}$$

The dependent variable is the four nominal support patterns from year-by-year LCA. To avoid imposing an ordinal ranking on nominal classes, the main estimation conducts all pairwise comparisons: for each class pair (k, l), it retains only household-years in these two classes, constructs a binary indicator of “belonging to class k,” and estimates a linear probability model. The key explanatory variables are the interactions of lockdown-intensity group with the post period. Define the post dummy $Post_t (=1 \text{ when survey year } t \geq 2020)$ and the interactions $Long_j \times Post_t$ and $Med_j \times Post_t$, which respectively measure the post-pandemic increments of the long- and medium-lockdown groups relative to the short-lockdown group, with the short-lockdown group and the pre-pandemic period jointly forming the baseline. Response start and end dates are compiled from provincial Health Commission announcements. X_{ijt} denotes the controls. Because urban/rural status, average schooling, and the female-member share are nearly time-invariant and their contemporaneous values may be endogenous to lockdown, they enter at their pre-pandemic baseline values (2018 preferred, 2016 if missing) interacted with year fixed effects; α_i and λ_t are household and year fixed effects; β_L and β_M are the multi-group DID estimators, capturing the average change in

the probability of belonging to class k (relative to class l) after versus before the pandemic in the long (medium) lockdown group; multiplying by 100 yields percentage-point changes. Standard errors are clustered at the baseline province. In addition, using the medium-lockdown group as comparison and retaining only medium- and long-lockdown observations, we estimate:

$$y_i^{(k,l)}jt = \delta(Long_j \times Post_t) + \gamma'X_{ijt} + \alpha_i + \lambda_t + \varepsilon_{ijt}$$

where Δ is the incremental effect of long relative to medium lockdown, testing whether the effect varies with lockdown intensity.

(2) Event study. Taking the pre-pandemic year 2018 as the reference period, we allow treatment effects to vary by year:

$$y_i^{(k,l)}jt = \sum_{\tau \neq 2018} [\theta_{\tau}^L(Long_j \times 1[t = \tau]) + \theta_{\tau}^M(Med_j \times 1[t = \tau])] + \gamma'X_{ijt} + \alpha_i + \lambda_t + \varepsilon_{ijt}$$

$\tau \in 2016, 2020, 2022$ indexes event time; the 2016 terms are pre-treatment coefficients used to test parallel trends, and θ_{2016}^L and θ_{2016}^M are matched by the post-period coefficients, which characterize when the effect emerges and whether it persists. A placebo test on pre-pandemic observations with a pseudo-treatment year of 2018 provides a supplementary check.

To satisfy conditional parallel trends and keep treatment status time-invariant, lockdown intensity is always assigned by the household's pre-pandemic baseline province (preferring 2018, falling back to 2016 or earlier) and held fixed across all four waves. About 10% of households resided in a different province in 2020 or 2022 than at baseline; we retain these households but do not update their treatment upon migration, so that migration decisions are not endogenous to the pandemic.

We address three potential violations of parallel trends in turn. First, time-varying treatment induced by cross-provincial migration is ruled out by baseline-province assignment. Second, differential shocks from regional pandemic severity and economic development are absorbed by household and year fixed effects and baseline-covariate-by-year interactions; in robustness checks we further add provincial linear time trends, provincial log GDP per capita, a continuous-lockdown-days dose-response specification, and the exclusion of Hubei, the province hardest hit and locked down longest. Third, treatment endogeneity and reverse causality are ruled out at the design stage by the centrally mandated Level-I response and exogenous initial exposure.

Because support items with consistent definitions were not systematically collected until 2016, we assess parallel trends with three types of evidence: the event-study pre-treatment coefficients; a placebo test on pre-pandemic observations that assigns a pseudo-treatment timing of 2018; and a pre-treatment balance test using household intergenerational-structure and demographic variables available in 2010 and 2014. Because treatment varies only across 31 provinces, in addition to province-clustered standard errors we report a wild cluster bootstrap for robust inference with a small number of clusters.

3.4 Sample characteristics

Table 2 reports the basic characteristics of the analytical sample and their change across four waves. The final sample contains 34,118 household-year observations from 10,067 households across 31 provincial-level regions, with 10,072 (2016), 9,225 (2018), 7,617 (2020), and 7,204 (2022) observations, forming an unbalanced panel. The urbanization rate rises modestly from 51.7% in 2016 to 56.2% in 2022; members' average schooling rises from 8.91 to 10.52 years; average age falls from 39.15 to 36.11 years; household size stays small at 2.2 – 2.5 persons; and the female share

is about 0.50 in every wave. Overall, the sample's demographic and socioeconomic characteristics change smoothly across the pandemic threshold, with no structural break coinciding with the pandemic timing.

Table 2. Sample characteristics by wave

Variable	Overall	2016	2018	2020	2022
Observations (household-years)	34,118	10,072	9,225	7,617	7,204
Urban share	0.533	0.517	0.526	0.537	0.562
Mean education (yrs)	9.52	8.91	9.13	9.84	10.52
Female member share	0.498	0.499	0.503	0.500	0.489
Mean age	37.75	39.15	37.80	37.39	36.11
Household size	2.34	2.27	2.47	2.40	2.19

Notes: The unit is the household-year. Cells report variable means for each wave; the “Overall” column reports the mean for the four-wave pooled sample.

4 Main Results

4.1 Descriptive trends in intergenerational support patterns

Figure 1 plots how the distribution of the four support patterns evolves across short-, medium-, and long-lockdown regions. Before the pandemic (2016–2018), the three groups move in broadly the same direction for each pattern, with nearly parallel trajectories, limited level differences, and no systematic widening over time. After the pandemic begins, the between-group trajectories diverge, and this divergence does not appear immediately at the outset of the shock but unfolds gradually over time.

Taking the Reciprocal type, its share in long-lockdown regions falls from 48.2% in 2016 to 43.7% in 2018, is 43.3% in 2020 (essentially unchanged from 2018), and rebounds to 47.3% in 2022, close to its pre-pandemic level; in short- and medium-lockdown regions the 2022 shares are 35.6% and 41.7%, still close to their 2020 levels and not back to pre-pandemic values. Correspondingly, the 2022 shares of Interactive and Filial-support are both lower in long-lockdown than in short-lockdown regions (by 5.0 and 5.2 percentage points): the longer the lockdown, the more households concentrate in two-way reciprocity and the more one-way support patterns decline. The Detached share fluctuates similarly across the three groups, dipping briefly in 2020 and returning to its original level by 2022, without rising persistently in the more strictly locked-down regions.

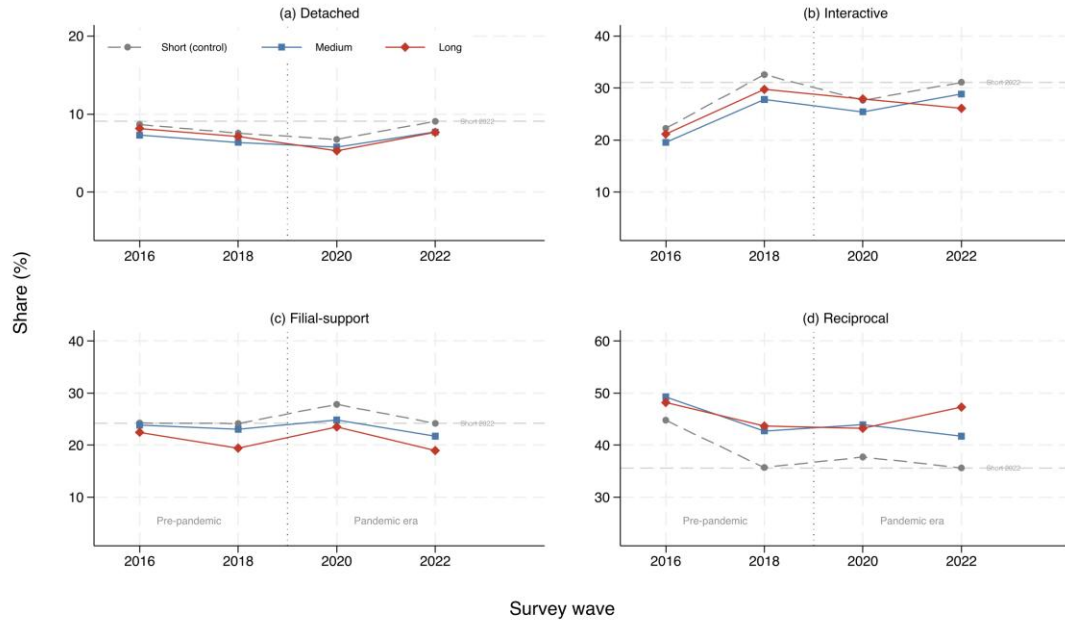


Figure 1. Changes in the shares of the four intergenerational support patterns across lockdown-intensity groups

Notes: Each panel's vertical axis uses a constant 10-percentage-point tick interval. Because baseline shares differ substantially across patterns, the displayed vertical range is set separately by panel to make group trajectories visible; the three curves within a panel therefore share one scale and are directly comparable, whereas vertical scales differ across panels, so cross-panel comparisons should rely on axis values rather than visual steepness. The light-gray horizontal dashed line marks the 2022 value of the comparison (short-lockdown) group, and the vertical dotted line marks the pandemic divide (2019).

4.2 Effects of lockdown intensity on support patterns

Figure 2 reports, as coefficient plots in two panels, the multi-group DID point estimates and 95% confidence intervals for the six pairwise comparisons. Panel A uses the short-lockdown group as comparison and estimates Long×Post and Medium×Post; Panel B uses the medium-lockdown group as comparison and estimates Long×Post.

As Figure 2 shows, in Panel A the confidence intervals for Long×Post lie entirely to the right of zero only for Reciprocal vs Interactive and Reciprocal vs Filial-support and cross zero for the other four comparisons, while Medium×Post avoids crossing zero only for Reciprocal vs Filial-support. In Panel B, only the interval for Reciprocal vs Interactive lies entirely to the right of zero, that for Reciprocal vs Filial-support marginally crosses zero, and the other four again cross zero. This is the overall visual pattern; specific coefficients, magnitudes, and significance are reported in Table 3.

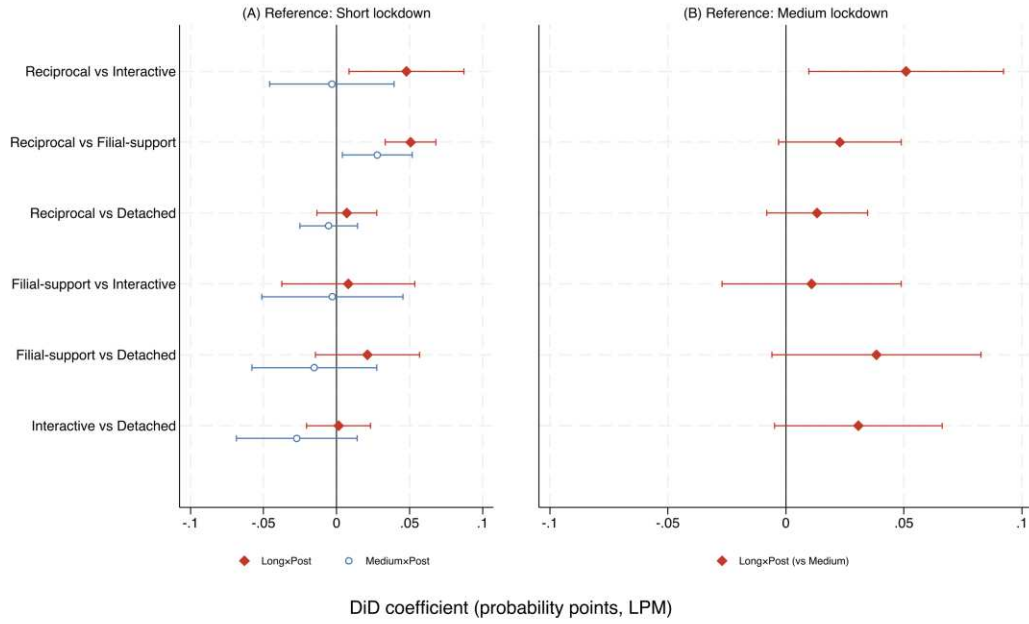


Figure 2. Multi-group DID coefficient plots

Notes: Dots are multi-group DID interaction coefficients and whiskers are 95% confidence intervals; standard errors are clustered at the baseline province (31 clusters); the vertical zero line denotes no effect; the vertical axis lists the six pairwise comparisons. Panel A uses the short-lockdown group as comparison and reports both Long $\times$ Post and Medium $\times$ Post; Panel B retains only medium- and long-lockdown observations, uses the medium group as comparison, and reports Long $\times$ Post. All models are linear probability models that absorb household and year fixed effects and include baseline-covariate-by-year interactions.

Table 3 reports the corresponding coefficients; columns (1) – (6) are the six pairwise comparisons in order. Every column absorbs household and year fixed effects and includes interactions of pre-pandemic baseline covariates with year fixed effects; robust standard errors clustered at the baseline province are in parentheses.

Panel A of Table 3 reports the main effects. Long $\times$ Post is significantly positive only for Reciprocal vs Interactive and Reciprocal vs Filial-support: relative to short-lockdown regions, long-lockdown regions raise the post-pandemic probability of being Reciprocal (rather than Interactive or Filial-support) by about 4.8 and 5.1 percentage points. Relative to the Reciprocal baseline shares of about 62% and 65% in the two subsamples, this amounts to roughly an 8% increase in the relative probability of being Reciprocal, a clear economic magnitude.

The Long coefficients for the other four comparisons are all insignificant, indicating that lockdown neither moves Detached households into any connected type nor induces transitions between Interactive and Filial-support.

Medium $\times$ Post is significant only for Reciprocal vs Filial-support and insignificant otherwise, showing that medium-intensity lockdown has a limited overall effect and that reconfiguration is concentrated in the regions locked down longest.

Panel B retains only medium- and long-lockdown observations and uses the medium group as comparison. The long-versus-medium difference is significantly positive at 5% for Reciprocal vs Interactive and marginally positive at 10% for Reciprocal vs Filial-support, and insignificant for the other four. Together with Panel A, the effect of lockdown increases with intensity and is concentrated in the regions locked down longest. This gradient—increasing in intensity and appearing only under

prolonged lockdown—is consistent with the theoretical argument in Section 2 that the consequences of a shock depend on its intensity and that only a sufficiently strong, persistent shock overcomes the inertia of support arrangements.

Table 3. Effects of lockdown intensity on intergenerational support patterns

Variables	Reciprocal vs Interactive	Reciprocal vs Filial- support	Reciprocal vs Detached	Filial- support vs Interactive	Filial- support vs Detached	Interactive vs Detached
Panel A: Short-lockdown comparison (full sample; short = baseline)						
Long lockdown × Post	0.0479** (0.0201)	0.0507*** (0.0088)	0.0071 (0.0105)	0.0080 (0.0232)	0.0211 (0.0182)	0.0014 (0.0112)
Medium lockdown × Post	-0.0032 (0.0217)	0.0279** (0.0122)	-0.0055 (0.0101)	-0.0028 (0.0246)	-0.0153 (0.0218)	-0.0272 (0.0212)
Urban (baseline) × 2016	0.0280 (0.0258)	0.0097 (0.0201)	0.0017 (0.0098)	0.0063 (0.0318)	0.0244 (0.0275)	0.0026 (0.0236)
Urban (baseline) × 2020	0.0343 (0.0204)	0.0243 (0.0155)	0.0017 (0.0105)	0.0029 (0.0213)	-0.0240 (0.0164)	-0.0368 (0.0270)
Urban (baseline) × 2022	0.0258 (0.0216)	0.0446** (0.0211)	0.0350** (0.0139)	-0.0269 (0.0321)	0.0040 (0.0260)	0.0318 (0.0363)
Education (baseline) × 2016	0.0127*** (0.0025)	0.0050** (0.0018)	0.0045** (0.0017)	0.0098*** (0.0022)	0.0058 (0.0045)	0.0015 (0.0025)
Education (baseline) × 2020	-0.0022 (0.0017)	-0.0074** (0.0031)	-0.0048** (0.0018)	0.0048* (0.0024)	0.0018 (0.0022)	-0.0043 (0.0026)
Education (baseline) × 2022	-0.0011 (0.0033)	-0.0119*** (0.0033)	-0.0067*** (0.0018)	0.0059* (0.0034)	0.0011 (0.0044)	-0.0001 (0.0036)
Female share (baseline) × 2016	0.1417*** (0.0240)	0.0603* (0.0300)	0.0564 (0.0383)	0.0231 (0.0325)	-0.0639** (0.0306)	0.0348 (0.0359)
Female share (baseline) × 2020	0.1411*** (0.0307)	0.0691* (0.0399)	0.0341* (0.0199)	-0.0055 (0.0271)	0.0037 (0.0313)	0.0059 (0.0323)
Female share (baseline) × 2022	0.1003*** (0.0358)	0.0566* (0.0286)	0.0584 (0.0420)	0.0427 (0.0272)	0.1369*** (0.0439)	0.0363 (0.0331)
Observations	21,123	19,944	14,109	13,461	7,132	8,111
R ²	0.472	0.480	0.595	0.466	0.545	0.537
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline covariates × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Medium-lockdown comparison (medium/long sample only)						
Long lockdown (vs. medium)	0.0509** (0.0210)	0.0229* (0.0132)	0.0132 (0.0109)	0.0109 (0.0194)	0.0384 (0.0226)	0.0307 (0.0181)
Observations	14,675	13,867	10,003	8,788	4,594	5,300
R ²	0.470	0.465	0.585	0.465	0.551	0.531

Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline covariates $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *, **, and *** denote significance at the 10%, 5%, and 1% levels. Robust standard errors clustered at the baseline province (31 clusters) are in parentheses. Each column is a linear probability model on the household-year subsample of the two corresponding support patterns, and the coefficient is the change in the probability of belonging to the target type. All columns absorb household and year fixed effects and include interactions of pre-pandemic baseline values of urban/rural status, average schooling, and female-member share with year fixed effects (2018 is the base year, so no “ $\times 2018$ ” term appears).

4.3 Event study

This section uses an event-study approach to conduct, within a single framework, both the identification diagnostic for parallel trends and the dynamic characterization of treatment effects. With the pre-pandemic year 2018 as reference, the model estimates period-by-period coefficients for the long- and medium-lockdown groups relative to the short-lockdown group in 2016, 2020, and 2022; Figure 3 reports the dynamic results for all six pairwise comparisons.

As Figure 3 shows, the six comparisons diverge clearly. Panel (a), Reciprocal vs Interactive, follows a three-stage trajectory: before the pandemic (2016) both the long- and medium-lockdown curves lie close to zero with no divergence; in the shock year (2020) they still fluctuate around zero; and by the second post period (2022) the long-lockdown curve moves clearly upward with its confidence interval entirely above zero, whereas the medium curve stays around zero throughout with no systematic deviation.

In panel (b), Reciprocal vs Filial-support, the long-lockdown curve is significantly below zero before the pandemic—that is, a negative differential pre-trend, with long-lockdown regions less likely to be Reciprocal (relative to Filial-support) ex ante—but reverses to positive after the pandemic (about 0.043 in 2022, marginal at 10%). This is a trend reversal rather than a continuation of the pre-trend, consistent with a causal interpretation in which lockdown drives a shift to reciprocity. Coefficients in the other four panels stay broadly near zero, with confidence intervals crossing zero except for a slightly positive medium-group pre-coefficient in Reciprocal vs Detached. Across the six panels, the effect is absent in the shock year, emerges only in the medium run, and exhibits no same-direction systematic divergence beforehand, consistent with the Section 2 argument about the inertia and gradual adjustment of support patterns.

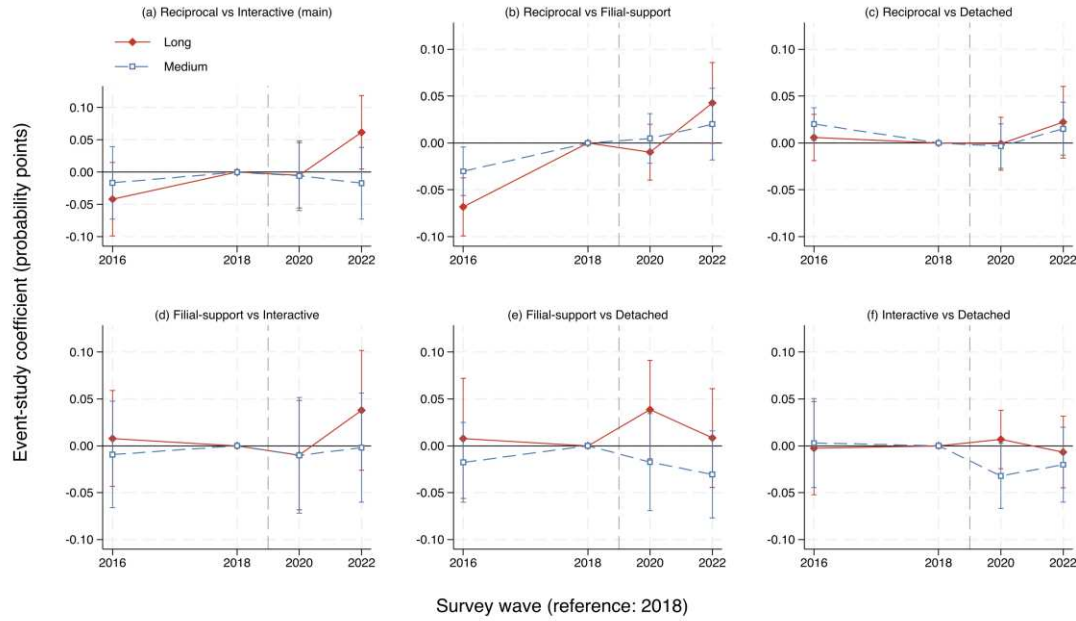


Figure 3. Event-study coefficient plots for the six pairwise comparisons

Notes: Each panel corresponds to one pairwise comparison: (a) Reciprocal vs Interactive, (b) Reciprocal vs Filial-support, (c) Reciprocal vs Detached, (d) Filial-support vs Interactive, (e) Filial-support vs Detached, (f) Interactive vs Detached. Red solid diamonds mark the long-lockdown group and blue open squares with dashed lines the medium-lockdown group; whiskers are 95% confidence intervals with standard errors clustered at the baseline province (31 clusters); the light-gray vertical dashed line marks the pandemic divide (end of 2019, between the 2018 reference and the first post-pandemic 2020 wave), and 2018 is the reference period (coefficient set to zero).

4.4 Robustness checks

To confirm that the baseline results do not hinge on a particular modeling choice or on chance, this section conducts robustness checks from six angles.

4.4.1 Controls, sample, continuous intensity, provincial trends, and macro controls

Table 4 summarizes seven specifications. Panel A varies controls and samples: the main effect is about 4.8 percentage points and is virtually unchanged with no controls at all, indicating that the result does not depend on the choice of controls. Note that variables such as household size, age, and health themselves change after lockdown and lie on the outcome path of the lockdown effect (bad controls); including them would absorb effects properly attributable to lockdown, so the main specification excludes them and includes them only in sensitivity analyses. When added, the effect falls to about 3.0 percentage points (marginal at 10%), exactly consistent with this reasoning.

On the sample side, after excluding Hubei—the province hardest hit and locked down longest—the effect is about 4.3 percentage points and still significant at 5%, showing that the result is not driven by this extreme province. Replacing the three-group treatment with continuous Level-I response days, each additional lockdown day raises the probability of shifting to reciprocity by about 0.23 percentage points, with the effect rising monotonically in duration—a dose–response relationship.

Panel B further adds provincial linear time trends and provincial log GDP per capita to rule out confounding from differential development trajectories and macroeconomic levels; the resulting effects (about 4.7 and 4.8 percentage points) match the main specification.

Table 4. Robustness: controls, sample, continuous intensity, provincial trends, and macro controls (Reciprocal

vs Interactive)

Notes: *, **, and *** denote significance at the 10%, 5%, and 1% levels. Robust standard errors clustered at the baseline province (31 clusters) are in parentheses. Each column is a linear probability model that absorbs household and year fixed effects and includes baseline-covariate-by-year interactions; the dependent variable is the binary Reciprocal vs Interactive indicator. Panel A varies the control set and sample, and Panel B adds provincial linear time trends and provincial macro controls.

Specification	Coef. (SE)	p-value	N
Panel A: Control set, sample and continuous intensity			
(1) Baseline (baseline covariates × year FE)	0.0479** (0.0201)	0.024	21,123
(2) No controls	0.0483** (0.0206)	0.026	21,473
(3) Full controls (potential mediators)	0.0301* (0.0152)	0.057	21,075
(4) Exclude Hubei (hardest-hit, longest lockdown)	0.0433** (0.0207)	0.045	20,829
(5) Continuous lockdown days × Post (per day)	0.0023*** (0.0005)	<0.001	21,123
Panel B: Province-specific trends and macro controls			
(6) + Province linear time trend	0.0471** (0.0227)	0.047	21,123
(7) + ln(provincial GDP per capita)	0.0475** (0.0202)	0.025	21,123

4.4.2 Alternative cutoffs and the donut specification

Under the three-group split by lockdown days, the cutoffs between medium and short groups (74 and 76 days) differ by only two days, so the grouping may be somewhat arbitrary. We examine this in two ways (Table 5): first, we re-group using several alternative cutoff pairs (75/79, 70/80, 74/83); second, we drop provinces near the cutoffs with similar durations (a donut specification), comparing only groups far enough apart in duration, so that borderline province assignment cannot drive the result.

The long-lockdown effect is significantly positive under all seven specifications: for Reciprocal vs Interactive it ranges 4.8–7.3 percentage points with p between 0.0002 and 0.026, and for Reciprocal vs Filial-support p is below 0.001 in every specification. The stricter the cutoff—and the more the long group is concentrated in the longest-locked provinces—the larger rather than smaller the effect, consistent with the continuous-day dose–response relationship. The main effect thus depends neither on the cutoff values nor on provinces adjacent to the cutoffs.

Table 5. Robustness: alternative grouping cutoffs and the donut specification

Specification (cutoffs / sample)	Long×Post: Reciprocal vs Interactive	Long×Post: Reciprocal vs Filial-support
(0) Baseline cut 74/76	0.0479** (0.0201)	0.0507*** (0.0088)
(A1) Alternative cut 75/79	0.0690*** (0.0194)	0.0564*** (0.0081)
(A2) Alternative cut 70/80	0.0558** (0.0208)	0.0576*** (0.0087)
(A3) Alternative cut 74/83 (Long ≥97 days)	0.0728*** (0.0168)	0.0632*** (0.0103)
(B1) Donut: drop 74/76	0.0493** (0.0200)	0.0527*** (0.0089)
(B2) Donut: drop 76/79	0.0536** (0.0204)	0.0569*** (0.0084)
(B3) Strict donut: ≤68 vs ≥83 days	0.0567** (0.0210)	0.0580*** (0.0092)

Notes: *, **, and *** denote significance at the 10%, 5%, and 1% levels. Robust standard errors clustered at the baseline province (31 clusters) are in parentheses. Each cell reports the Long×Post coefficient from a linear probability model that absorbs household and year fixed effects and includes baseline-covariate-by-year interactions. Panel A re-splits the three groups using alternative day cutoffs; Panel B drops provinces near the cutoffs (donut), with B3 retaining only the two extreme groups with the largest duration gap.

4.4.3 Level of inference: Provincial clustering and wild cluster bootstrap

The key explanatory variable varies only at the province level, and households in the same province face the same lockdown policy and common regional shocks, so their disturbances are correlated. Ordinary household-level robust standard errors would ignore this within-province correlation and understate uncertainty (Moulton, 1990); all models therefore cluster standard errors at the baseline province. With only 31 provinces and conventional clustering potentially optimistic in small samples, we further report the most conservative assessment using a wild cluster bootstrap-t (Table 6).

Point estimates are identical across the three methods; only uncertainty estimates differ. For Reciprocal vs Interactive, the heteroskedasticity-robust p is 0.005, the province-clustered p is 0.024, and under the most conservative wild cluster bootstrap p=0.094; Reciprocal vs Filial-support is highly significant under all three. For the event-study T+2 (2022) term, the province-clustered p is 0.042 and the wild bootstrap p about 0.11 (0.107 with Rademacher weights, 0.111 with Webb weights), with stable direction and magnitude but a value slightly above the 10% critical level.

Table 6. Robustness: province-clustered CRVE versus wild cluster bootstrap

Model (Long-lockdown effect)	Coef.	Hetero-robust p	Cluster-prov. p (31)	Wild p (Rademacher)
Main: Reciprocal vs Interactive, Long×Post	0.0479	0.005	0.024	0.094
Reciprocal vs Filial-support, Long×Post	0.0507	0.003	<0.001	0.002
Event-study T+2 (2022), Recip. vs Interactive	0.0615	0.010	0.042	0.107

Notes: Point estimates are identical across the three inference methods; only standard errors and p-values differ. Column 3 reports heteroskedasticity-robust p-values, column 4 CRVE p-values clustered at the baseline province (31 clusters; our main inference), and column 5 wild cluster bootstrap-t p-values (boottest, 9,999 replications, Rademacher weights); Webb weights give similar values (0.088, 0.001, 0.111, respectively). All models absorb household and year fixed effects and use pre-pandemic baseline urban/rural status, average schooling, and female-share interacted with year FE as covariates.

4.4.4 LCA classification uncertainty: Pseudo-class draws

Because the outcome pattern comes from year-by-year LCA, ignoring the information that a household may lie between two classes and be classified with uncertainty could understate standard errors. We assess this with pseudo-class draws (Asparouhov & Muthén, 2014): between the two classes involved in each comparison, we randomly assign class membership according to posterior probabilities, repeat this 300 times, re-estimate the main specification each time, and combine within- and between-draw variation using Rubin's rules.

As Table 7 shows, the overall pattern is unchanged once classification uncertainty is accounted for: the Rubin-combined effect for Reciprocal vs Filial-support is about 4.3 percentage points and significant at 5% in 92% of the 300 draws, quite robust; for Reciprocal vs Interactive it is about 4.1 percentage points and always positive, but only about half of draws reach 5% significance and the combined result is marginal at 10%; the other four comparisons and all medium-lockdown terms are insignificant after combination with no stable direction.

Table 7. Robustness to LCA classification uncertainty: pseudo-class draws (B=300)

Comparison · term	Hard-assignment coef.	Hard-assignment p	Rubin coef.	Rubin SE	Rubin p	Empirical 95% range of draws	Share sig. at 5%
Reciprocal vs Interactive (RvI) · Long × Post	0.0479	.024	0.0410	0.0228	.073	[0.0202, 0.0619]	52%
Reciprocal vs Interactive (RvI) · Medium × Post	-0.0032	.885	-0.0019	0.0225	.933	[-0.0208, 0.0156]	0%

						0.0153]	
Reciprocal vs Filial-support (RvF) · Long × Post	0.0507	<0.001	0.0425	0.0176	.016	[0.0181,	92%
						0.0653]	
Reciprocal vs Filial-support (RvF) · Medium × Post	0.0279	.030	0.0150	0.0190	.428	[-0.0068,	11%
						0.0368]	
Reciprocal vs Detached (RvD) · Long × Post	0.0071	.504	0.0083	0.0107	.442	[0.0046,	0%
						0.0121]	
Reciprocal vs Detached (RvD) · Medium × Post	-0.0055	.590	-0.0054	0.0104	.608	[-0.0089, -	0%
						0.0018]	
Filial-support vs Interactive (FvI) · Long × Post	0.0080	.732	-0.0048	0.0218	.825	[-0.0262,	0%
						0.0179]	
Filial-support vs Interactive (FvI) · Medium × Post	-0.0028	.909	-0.0050	0.0247	.838	[-0.0271,	0%
						0.0175]	
Filial-support vs Detached (FvD) · Long × Post	0.0211	.256	0.0130	0.0237	.585	[-0.0153,	9%
						0.0454]	
Filial-support vs Detached (FvD) · Medium × Post	-0.0153	.488	-0.0172	0.0270	.525	[-0.0436,	2%
						0.0087]	
Interactive vs Detached (IvD) · Long × Post	0.0014	.901	0.0041	0.0215	.850	[-0.0223,	6%
						0.0347]	
Interactive vs Detached (IvD) · Medium × Post	-0.0272	.208	-0.0306	0.0262	.242	[-0.0605, -	19%
						0.0033]	

Notes: Hard-assignment coefficients/p are the main results under posterior-mode hard assignment; Rubin coefficients, standard errors, and p-values combine within- and between-draw variance across 300 pseudo-class draws per Rubin's rules; the “empirical 95% range of draw coefficients” is the 2.5th–97.5th percentile range of the 300 point estimates, describing their sampling distribution rather than a Rubin confidence interval, and hence need not align exactly with Rubin p-value significance; the “share significant at 5%” is the fraction of the 300 draws significant at 5%. Standard errors are clustered at the baseline province (31 clusters); each model absorbs household and year fixed effects and baseline-covariate-by-year FE interactions.

4.4.5 Multiple-testing correction: Bonferroni and Benjamini–Hochberg FDR

We test six pairwise comparisons at once, and the more tests conducted, the higher the probability that a “significant” result arises from random variation alone. To guard against multiple-comparison false positives, Table 8 reports the raw p-values, Bonferroni-adjusted p-values, and Benjamini–Hochberg (BH) false-discovery rates together: Bonferroni is the most stringent, raising the significance threshold sixfold to control the family-wise error rate (FWER); BH is more lenient, controlling the expected share of false discoveries among results declared significant (FDR).

The long-lockdown raw p for Reciprocal vs Filial-support is below 0.001 and remains below 0.001 after both adjustments; for Reciprocal vs Interactive the raw p is 0.024, rising to 0.142 after Bonferroni (no longer conventionally significant) and 0.071 under BH-FDR (marginal at 10%); the other four comparisons and all medium-lockdown terms are insignificant after adjustment.

Table 8. Multiple-testing correction: raw, Bonferroni, and BH-FDR p-values for the six pairwise comparisons

Comparison (Long × Post)	Coef. (SE)	Raw p	Bonferroni p	BH-FDR p	Medium coef.	Medium p
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Reciprocal vs Interactive	0.0479 (0.0201)	0.024	0.142	0.071	-0.0032	0.885
Reciprocal vs Filial-support	0.0507 (0.0088)	<0.001	<0.001	<0.001	0.0279	0.030
Reciprocal vs Detached	0.0071 (0.0105)	0.504	1.000	0.757	-0.0055	0.590
Filial-support vs Interactive	0.0080 (0.0232)	0.732	1.000	0.878	-0.0028	0.909
Filial-support vs Detached	0.0211 (0.0182)	0.256	1.000	0.511	-0.0153	0.488
Interactive vs Detached	0.0014 (0.0112)	0.901	1.000	0.901	-0.0272	0.208

Notes: Raw p-values are robust p-values clustered at the baseline province (31 clusters); Bonferroni $p = \min(1, 6 \times \text{raw } p)$, controlling the family-wise error rate; BH-FDR is the Benjamini–Hochberg false-discovery rate. Each model absorbs household and year fixed effects and baseline-covariate-by-year FE interactions.

4.4.6 Parallel-trend sensitivity: HonestDiD

DID requires that, absent the pandemic, the groups would have followed similar trajectories—the parallel-trends assumption. Before the 2018 reference period we have only the 2016 wave, making a joint test with multiple pre-coefficients difficult; we therefore conduct sensitivity analysis using the HonestDiD approach of Rambachan and Roth (2023). Rather than requiring parallel trends to hold exactly, it allows post-period trends across groups to violate parallel trends by no more than M_{bar} times the maximum pre-period deviation ($M_{\text{bar}}=0$ is exact parallel trends, and larger values are more permissive), constructs robust confidence intervals, and examines when they begin to contain zero as M_{bar} rises.

Following the event-study setup of Section 4.3, we construct 95% and 90% robust intervals over $M_{\text{bar}} \in [0, 1]$ for the contemporaneous term $T+1$ and the end term $T+2$ (Figure 4).

As Figure 4 shows, the $T+1$ robust interval contains zero at every M_{bar} , consistent with “no contemporaneous effect in the shock year”; under exact parallel trends ($M_{\text{bar}}=0$) the $T+2$ interval is $[0.005, 0.118]$ and still excludes zero, with breakdown values M_{bar}^* of about 0.10 (95%) and 0.15 (90%)—the end-period effect vanishes only if post-period trend deviations reach about 10–15% of the maximum pre-period fluctuation. This tolerance band is fairly narrow, mainly because the $T+2$ point estimate is itself marginal at 5% and because the single pre-period wave, used as the yardstick for deviation, makes the intervals conservative.

Importantly, our identification does not rest on pre-trends alone: the medium-lockdown group shows no significant effect throughout, providing a negative control along the intensity dimension; together with pre-pandemic balance, the placebo test (Section 4.3), alternative intensity measures, and provincial linear trends (Sections 4.4.1–4.4.2), the scope for a differential pre-trend that could overturn the result is quite limited. On balance, the main effect holds robustly under exact parallel trends and its direction does not depend on a particular specification, but it is sensitive to moderate violations of parallel trends; we therefore characterize it as causal evidence that is “robust in direction and moderate in strength.”

Across the six sets of checks, the core effect is consistent in direction under variation in intensity measure, grouping cutoffs, sample composition, covariate specification, provincial trends, and inference method, and strengthens monotonically with lockdown duration. The most stringent significance corrections and classification-uncertainty checks reduce the precision of Reciprocal vs Interactive to marginal, but Reciprocal vs Filial-support holds under every approach, confirming the reliability of the baseline regression.

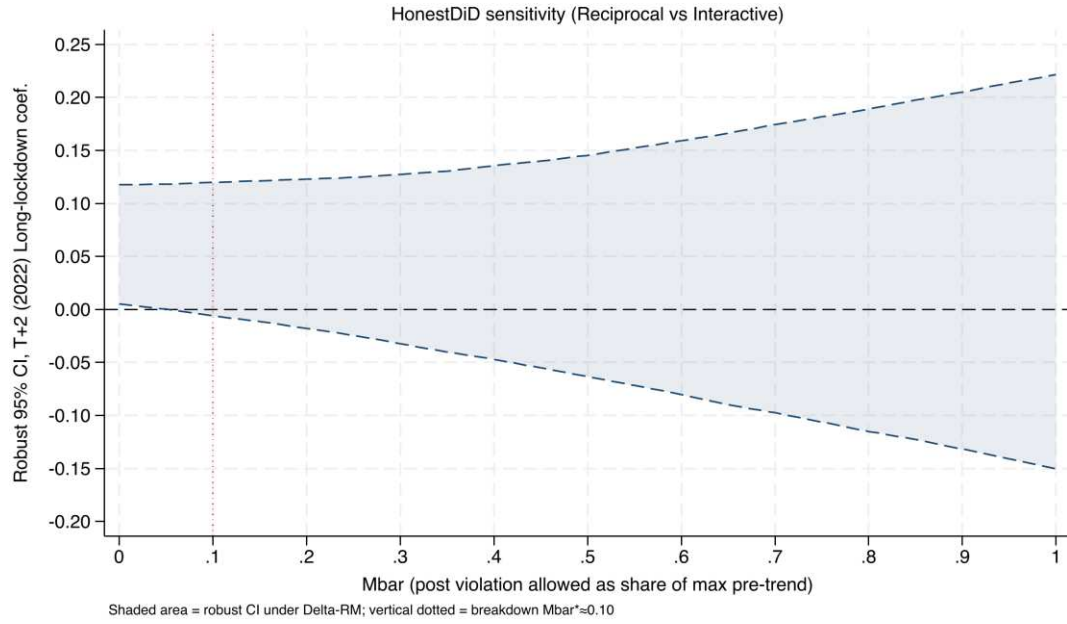


Figure 4. HonestDiD parallel-trends sensitivity: robust confidence intervals for the main T+2 identification (ARM)

Notes: Shaded areas are 95% robust confidence intervals under relative-magnitude restrictions (ΔRM); the vertical dashed line marks the breakdown value $Mbar^* \approx 0.10$. The T+1 robust interval crosses zero at every Mbar and is not plotted separately.

4.5 Heterogeneity by household income

Average effects may mask differential consequences of lockdown across households of different income levels. Using terciles of pre-pandemic baseline household income per capita, we divide the sample into low-, middle-, and high-income households and estimate the treatment effect of the long-lockdown group within each income class through a triple-interaction model, absorbing household and year fixed effects and including baseline-covariate-by-year interactions. Table 9 reports the full group-specific estimates for the six pairwise comparisons, and Figure 5 further presents each income group's dynamic trajectory through an event study.

Panel A of Table 9 shows two distinct patterns of income heterogeneity by transition direction. First, the shift toward two-way reciprocity is widespread: the long-lockdown effect for Reciprocal vs Interactive is 5.6, 4.5, and 4.2 percentage points in low-, middle-, and high-income households, respectively, all significantly positive, and the between-group interaction differences (middle–low, high–low) are both insignificant, indicating that this transition is not stratified by income; for Reciprocal vs Filial-support the effect is likewise significant in middle- and high-income households (5.7 and 6.2 percentage points) but insignificant among low-income households.

Second, the shift toward one-way upward filial support is highly concentrated in low-income households, with sharp between-group divergence: for Filial-support vs Detached the long-lockdown effect reaches 11.3 percentage points in low-income households, is close to zero in middle-income households, and even turns negative in high-income households (–2.4 points), for a high–low gap of –13.7 points (significant at 1%); for Filial-support vs Interactive it is 6.0 points in low-income households (significant at 5%) but –0.3 and –2.2 points in middle- and high-income households, for a high–low gap of –8.2 points (significant at 5%).

This also explains why the two Filial-support comparisons are insignificant in the main

regression: the effect exists only in the low-income subgroup and is diluted in the full-sample average. In sum, prolonged lockdowns broadly push households of all types toward two-way reciprocity, but only low-income households additionally move from detachment or pure interaction into one-way upward filial support.

Panel B further confirms that these low-income effects are incremental to long lockdown. In Filial-support vs Detached, long relative to medium lockdown additionally raises the probability that a low-income household enters Filial-support by 12.5 points, with a high–low gap of –12.9; in Filial-support vs Interactive, the corresponding low-income increment is 6.3 points and the high–low gap –8.5. Combined with Table 3, where the medium group shows no significant effect in most comparisons, the reconfiguration intensifies with lockdown strength and operates most strongly among the most resource-constrained households; such substantive reorganization appears only in households that are both locked down longest and least economically buffered.

Table 9. Household income heterogeneity: full-sample interaction models (Panel A uses short-lockdown comparison; Panel B uses medium-lockdown comparison)

Panel A: Reference = Short-lockdown group						
Outcome comparison	Treatment	Low (1)	Middle (2)	High (3)	Middle–Low (4)	High–Low (5)
Reciprocal vs Interactive	Long×Post	+0.056* (0.029)	+0.045** (0.019)	+0.042* (0.024)	-0.011 (0.015)	-0.014 (0.030)
	Med×Post	+0.001 (0.024)	-0.020 (0.033)	+0.008 (0.033)	-0.020 (0.038)	+0.008 (0.036)
Reciprocal vs Filial-support	Long×Post	+0.027 (0.018)	+0.057** (0.021)	+0.062*** (0.019)	+0.030 (0.022)	+0.036 (0.028)
	Med×Post	+0.002 (0.014)	+0.048*** (0.015)	+0.028 (0.021)	+0.047*** (0.014)	+0.027 (0.022)
Reciprocal vs Detached	Long×Post	+0.017 (0.023)	-0.000 (0.010)	+0.009 (0.017)	-0.017 (0.025)	-0.008 (0.025)
	Med×Post	+0.004 (0.011)	-0.008 (0.014)	-0.013 (0.015)	-0.012 (0.016)	-0.017 (0.014)
Filial-support vs Interactive	Long×Post	+0.060** (0.026)	-0.003 (0.034)	-0.022 (0.040)	-0.063* (0.032)	-0.082** (0.036)
	Med×Post	+0.006 (0.044)	+0.004 (0.031)	-0.012 (0.030)	-0.002 (0.038)	-0.017 (0.051)
Filial-support vs Detached	Long×Post	+0.113*** (0.024)	-0.001 (0.072)	-0.024 (0.029)	-0.114 (0.069)	-0.137*** (0.036)
	Med×Post	+0.054 (0.033)	-0.049** (0.022)	-0.030 (0.025)	-0.103*** (0.022)	-0.084** (0.031)
Interactive vs Detached	Long×Post	-0.004 (0.023)	+0.027 (0.029)	-0.024 (0.015)	+0.031 (0.043)	-0.020 (0.028)
	Med×Post	+0.007 (0.051)	-0.059** (0.025)	-0.023 (0.023)	-0.066 (0.060)	-0.030 (0.055)
Panel B: Reference = Medium-lockdown group (medium & long subsample)						
Outcome comparison	Long vs Med	Low (1)	Middle (2)	High (3)	Middle–Low (4)	High–Low (5)
Reciprocal vs Interactive	Long vs Med	+0.061** (0.029)	+0.049** (0.020)	+0.045* (0.025)	-0.012 (0.016)	-0.016 (0.031)

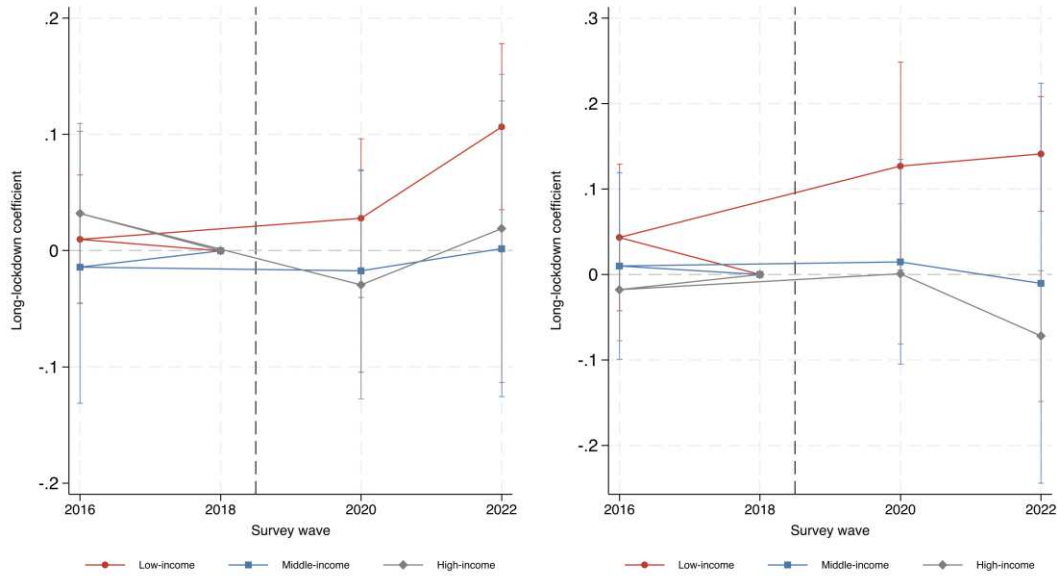
Reciprocal vs Filial-support	Long vs Med	+0.001 (0.020)	+0.031 (0.022)	+0.035 (0.023)	+0.030 (0.022)	+0.034 (0.029)
Reciprocal vs Detached	Long vs Med	+0.021 (0.023)	+0.006 (0.010)	+0.017 (0.018)	-0.015 (0.025)	-0.004 (0.025)
Filial-support vs Interactive	Long vs Med	+0.063** (0.026)	-0.001 (0.033)	-0.022 (0.036)	-0.064* (0.033)	-0.085** (0.037)
Filial-support vs Detached	Long vs Med	+0.125*** (0.027)	+0.011 (0.071)	-0.005 (0.038)	-0.114 (0.069)	-0.129*** (0.038)
Interactive vs Detached	Long vs Med	+0.019 (0.029)	+0.055* (0.029)	+0.012 (0.020)	+0.036 (0.042)	-0.006 (0.026)

Notes: Low/Middle/High are terciles of pre-pandemic baseline household income per capita. Panel A uses the short-lockdown group as comparison and reports the total long_post and med_post effects within each income group (middle- and high-income effects are linear combinations of the main and interaction terms) along with middle–low and high–low interaction differences. Panel B uses the medium group as comparison on medium/long observations only and reports the long-versus-medium effect. All models absorb household and year fixed effects and include baseline-covariate-by-year interactions; standard errors clustered at the baseline province (31 clusters), robust SE in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% levels.

The dynamic trajectories in Figure 5 provide further evidence on parallel trends and timing. With 2018 as reference, every income group's 2016 pre-coefficient is close to zero and insignificant, satisfying ex-ante parallel trends. After the pandemic, the long-lockdown effect for low-income households climbs steadily: in Filial-support vs Detached it rises from 0.127 in 2020 to 0.141 in 2022, whereas middle- and high-income households fluctuate around zero throughout, and between-group divergence widens over time rather than appearing as a one-time jump. This gradual, cumulative trajectory is consistent with a sustained-pressure logic and rules out a transitory-fluctuation explanation.

Two supplementary checks assess sensitivity to the grouping. First, replacing the terciles with the standardized continuous value of baseline log income per capita, the income interaction is -0.047 for Filial-support vs Detached and -0.030 for Interactive vs Detached (both significant at 1%): a one-standard-deviation increase in baseline income significantly attenuates the long-lockdown effect, in the same direction as the tercile result. Second, replacing income with terciles of baseline household net worth, all high–low differences are insignificant, indicating that this pattern divergence relates more to households' contemporaneous cash-flow buffers than to their stock of wealth.

In sum, lockdown's reshaping of intergenerational support exhibits a clear income gradient: concentration toward two-way reciprocity is widespread, whereas relying on one-way upward filial support as a household safety-net strategy appears mainly among economically vulnerable low-income households lacking external buffers. This asymmetric reconfiguration is consistent with the Section 2 argument that a family's mode of insurance depends on the resources it can mobilize.



Left: Ffilial-support vs Interactive; Right: Ffilial-support vs Detached

Figure 5. Event study × household income group: period-by-period long-lockdown effects (reference year 2018)

Notes: Reports period-by-period event-study coefficients for long lockdown with 2018 as reference (coefficient set to zero); dots are point estimates and whiskers 95% confidence intervals, with standard errors clustered at the baseline province (31 clusters); the vertical dashed line marks the pandemic divide (end of 2019). Low/middle/high groups are terciles of pre-pandemic baseline household income per capita, and each group's coefficients are linear combinations from the full-sample income-interaction model.

5 Conclusion and Discussion

5.1 Conclusion

Using the differential provincial lockdowns of COVID-19 as a quasi-experiment and drawing on four CFPS waves (2016 – 2022), we summarize six dimensions of intergenerational support into four patterns and apply multi-group DID, an event study, and income triple interactions to identify the causal effect of lockdown on support configurations, its dynamic path, and its resource heterogeneity.

We reach three conclusions. First, prolonged lockdowns significantly reconfigure households toward two-way reciprocity, with the effect increasing in intensity: relative to short-lockdown regions, long-lockdown regions raise the post-pandemic probability of being Reciprocal rather than Interactive or Filial-support by about 4.8 and 5.1 percentage points, the effect in medium-lockdown regions is limited, and continuous lockdown days show a monotonic dose – response relationship; meanwhile, the Detached share dips only briefly in the shock year without expanding persistently in more strictly locked-down regions, so the competing expectation of a broad shift toward detachment (H1.2) is not supported, the expectation of a shift toward reciprocity (H1.1) is supported, and H1—that the shock induces pattern transitions—is thereby borne out. Second, the effect is delayed: close to zero in the shock year (2020) and significant only by the second period (2022), with no systematic ex-ante differences and valid parallel trends, supporting the temporal-effect hypothesis H2. Third, the reconfiguration is stratified by household resources: concentration toward two-way reciprocity is widespread, whereas the shift to one-way upward filial support is highly concentrated in low-income households and weakens as income rises, corroborating the resource-

stratification hypothesis H3.

5.2 Discussion

Our first conclusion is that prolonged lockdown primarily induces a configuration-level reorganization of the intergenerational “support bundle,” rather than a change in the quantity of any single dimension, and that the extent of reorganization rises with lockdown intensity. This speaks to the literature on family risk sharing and intergenerational transfers. Where prior work focuses on changes in a single transfer or care dimension and on substitution between informal care and formal services (Bonsang, 2009; Van Houtven & Norton, 2004; Charles & Sevak, 2005), we show that the primary consequence of a prolonged shock is a recombination across economic, instrumental, and emotional support. The classic distinction between altruistic and exchange transfer motives is not mutually exclusive in our results but maps onto different support configurations: most households can sustain and strengthen two-way flows, reflected in the broad expansion of reciprocity, consistent with the view that families cohere under adversity and that social ties protect health (Walsh, 2003; Berkman et al., 2000). Of the two competing paths in Section 2, the evidence supports the path in which the crisis activates informal-insurance and reciprocity norms rather than the path in which the same shock contracts multidimensional support toward detachment: when formal services and market supply contract together, households turn inward to organize reciprocity rather than letting intergenerational ties dissolve. This direction of change is bound up with China's strong familist norms and the prevalence of intergenerational co-residence and proximate living, implying that which competing path prevails depends on the specific institutional and cultural context. Prior evidence documents the protective role of intergenerational support for Chinese older parents' well-being (Cong & Silverstein, 2008; Lei et al., 2015); we further show the form in which this protection is organized during a crisis, and the monotonic increase with lockdown duration provides population-economics evidence on the dose – response between exogenous shock intensity and family response.

Our second conclusion is a roughly two-year lag in configuration adjustment: near-unchanged in the shock year and significant only in the medium run, with parallel ex-ante trajectories. This dynamic feature refines the existing picture of pandemic family consequences. Where prior studies mostly report short-run changes in intergenerational contact or care during the pandemic, we suggest that support patterns are inertial: their reorganization depends on households' judgment of the shock's persistence and on the gradual reallocation of resources, and households systematically adjust their internal division of labor only once the shock is recognized as persistent and formal services remain absent (Elder, 1999; Settersten, 2020). Short-run cross-sections or single-wave follow-ups may therefore understate or misread the medium-run family consequences of a crisis, and assessing the family consequences of public-health emergencies requires an adequately long observation window.

Our third conclusion is that reconfiguration is stratified by resources: concentration toward two-way reciprocity is widespread, whereas the shift to one-way upward filial support is highly concentrated in low-income households. This echoes research on risk coping in poor households—households lacking external buffers rely more on intra-family self-protection (Morduch, 1994), which in our setting appears as low-income households falling back on adult children's one-way upward filial support, while upward care may impose additional costs by crowding out children's time and labor supply (Ettner, 1995). The family is thus not a homogeneous safety net, and its mode of insurance depends deeply on the economic resources it can mobilize; ignoring configuration and

resource heterogeneity yields the misleading conclusion that average effects cancel out. This pattern also resonates with the household resource constraints long faced by rural old-age support in China (Cai et al., 2012).

These findings carry clear policy implications. Families do perform an important risk-sharing function when public services contract, and in most households this takes the resilient form of two-way reciprocity; for low-income households, however, the crisis primarily activates adult children's one-way filial support, potentially at the cost of children's labor supply, consumption, and development, accumulating disadvantage within the family. This delineates the boundary of the family as a safety net: against the backdrop of population aging and family miniaturization, old-age and care responsibilities should not simply be pushed back onto families. Targeted income support for low-income and older households during crises, and the timely provision of community and formal care services, matter not only for older parents' welfare but also prevent crisis costs from accumulating onto the younger generation through intra-family transfers, keeping family insurance from becoming a transmission channel of within-family inequality.

This study also has limitations. First, identification relies on the exogeneity of provincial lockdown intensity; although the event study, pre-treatment balance, placebo, and parallel-trends sensitivity analyses all support identification, only a limited number of pre-pandemic waves are available and long-run parallel trends require longer panels to verify. Second, LCA portrays a continuous support mix through discrete types, and despite repeated-draw robustness checks, class boundaries retain statistical uncertainty. Third, we focus on the strong-shock setting of China's strict lockdowns, and extrapolation to public-health interventions of other intensities or to other institutional settings requires caution. Future research can examine, in longer panels, whether support configurations swing back after reopening and, combining consumption, time-use, and health data, further test the specific mechanisms through which configuration reconfiguration affects family welfare.

Conflict of Interest

The authors declare that they have no conflict of interest.

Data Availability Statement

The data that support the findings of this study are available from the China Family Panel Studies (CFPS), conducted by the Institute of Social Science Survey, Peking University, and are publicly accessible upon registration and application at <https://www.isss.pku.edu.cn/cfps/>. The Stata and R code used to construct the analysis sample and reproduce all results is available from the corresponding author on reasonable request.

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